THE

MEDICAL REPOSITORY.

VOL. I.—No. II.

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ADVERTISEMENT.

IN presenting their subscribers and the public with a second Number of the Medical Repository, the Publishers take pleasure in being able to announce that the undertaking has been so far prosperous, that they entertain no doubt of its failure from want of materials. The resources within their immediate controul, and those which are furnished them by the friendship and disinterested aid of able and diligent correspondents, have made it certain that the efficient patronage of their fellow-citizens of the United States, is all that is requisite to its full success. Of the truth of this the subscribers will probably admit no suspicion, when they observe that the number of pages, in the two publications already made, surpasses, by a third, what was promised in the original proposals.

But, a short delay has happened in the delivery of the present Number, the causes of which it is proper to explain. This has been partly occasioned by an unavoidable obstacle to the timely receipt of a very valuable article inserted in the original part of this work. But even this would not have retarded the delivery of this Number, but for unexpected and inevitable avocations on the part of the Printers. This circumstance delayed the completion of the impression for a week; an accident the like of which care will be taken to prevent in future. The effect of it might have been avoided now, by the omission of the Appendix; but it was thought that the important information it contains would well compensate for a tardiness of a few days.

Some doubt or misapprehension having occurred, as we are informed, relative to the 5th of the proposed conditions of publication, the publishers take this opportunity of explaining its intent more clearly.

Any subscriber is at liberty to stop his subscription at the time of the receipt of any Number of this publication; he paying, at the time, the sum required by the said 5th article, and giving notice to the publishers. After this he will receive an additional number and no more. The reason of this regulation is, to avoid the trouble of transmitting, and receiving back or losing, a number after the work is discontinued. The subscriber will, in fact, receive as many numbers as he shall have paid for; and the publishers will be allowed a reasonable notice of his having discontinued his subscription.

New-York, Nov. 7, 1797.

ERRATA.

The reader is requested to correct the following errors of the press.

Page 169, line 30, for *conserva* read conferva.

166, line 2 from the bottom, for *exerted* read excited.

170, line 29, insert *the* between *upon* and *gaseous*.

171, line 9 from the bottom, for *health* read healthy.

173, line 2, for *inquiries* read inquirers.

191, l. 11, from the bottom, for *fifty-handed* read hundred-handed.

MEDICAL REPOSITORY.

VOL. I.—No. II.

ARTICLE I.

ON THE DIGITALIS PURPUREA.

By JAMES MEASE, *M. D. Resident Physician of the Port of Philadelphia.*

THE *Digitalis Purpurea*, or Purple Fox Glove, is a medicine which, for some time past, stood high in the list of the Materia Medica, but, for various reasons, appears to have, in some measure, lost the character once formed of it. The frequent trials made by the judicious Dr. Withering, and the repeated success that has attended its use in his hands, certainly shew, that the opinion he entertained of it, as a valuable addition to the stock of medicinal plants, was well founded. Many other physicians have likewise spoken favourably of its good effects; but many instances of failure have also been recorded, and in cases too apparently favourable to its operation. I have seen it exhibited both in public and private practice, and have given it myself, sometimes with advantage, and at other times with little benefit, or obvious injury. The causes of this different success have never been fully ascertained; but the result of the present experience of this medicine, I believe, is rather unfavourable to its character. From my own reflections and observations, I am disposed to attribute the want of success of *digitalis* less to its own inert quality than to the circumstances attending its exhibition, which I shall point out in the following paper; and I shall also endeavour to ascertain the manner most proper to exhibit the medicine, and the cases most favourable to its operation.

The causes influencing the success of the *digitalis*, may be referred to the following heads.

1. The season of collecting the plant.
2. The part of the plant used.

3. The mode of curing it.
4. The adulteration of the plant.
5. The manner of exhibiting it.
6. The condition of the system at the time of use.

I. The *season of the year in which the plant is collected* will have a very considerable influence on the effects produced on the system. It is well known, that the active parts of vegetables possess the most power when the sap is present in them; and, for this reason those plants whose virtue resides in the root, are directed to be taken up early in the spring, before the sap has ascended: and, on the contrary, vegetables, whose medicinal quality exists in the upper parts of the plant, are directed to be used in summer. Accordingly, as Dr. Withering has found the leaves the most efficacious part, he directs them to be collected "about the time the "flowering stem has shot up, and when the blossoms are coming "forth." Contrary, however, to this rule, I have known the *digitalis* collected late in summer, and autumn; and it is not to be wondered if this, combined with other causes, should produce a disappointment to the expectations of the prescriber.

II. The *part of the plant employed* ought likewise to be attended to when forming a judgment of its virtues. The parts constantly used by Dr. Withering *are the small leaves;* and he is so cautious to have these alone, that he directs the *leaf stalk*, and even the *mid rib* to be thrown away. But without attending to this rule, I have known every part of the plant indiscriminately mixed, and exhibited in this state; and have heard an opinion of its virtues formed from the little effects produced by such a partial and unfair proceeding. I do not know that the root possesses any properties different from those of the small leaves, or that it would in any way prove injurious by being mixed therewith, otherwise than weaken the effect of the active part: yet, in the exhibition of a medicine, so celebrated as the present, in a disease that so often baffles the best directed efforts for a cure, every circumstance which may in the least influence its operation, should be strictly guarded against; and as long and repeated experience has convinced Dr. Withering of the superior efficacy of the small leaves to the other parts of the plant, it is certainly incumbent upon every prescriber to pay attention to this caution. I have known, however, every part used without distinction, except the root, both in a separate and combined state; and I have no doubt a neglect of this rule has been one common source of the failure of this medicine.

III. A frequent cause of the failure of *digitalis* may be attributed to a careless mode of preparing it for use. From its sensible qualities it appears to be of a volatile nature; and Dr. Withering, aware of this circumstance, directs it to be slightly dried

in the sun-shine, or on a tin pan before the fire, so as barely to allow its reduction to powder. There is great reason to believe, however, that its active quality has been often either entirely or greatly dissipated by being long dried by a hot fire, from a supposition, that this was requisite to permit its reduction to powder.

IV. The *adulteration of the plant* with other vegetables is a source of failure, to which I know some unsuccessful cases may be ascribed; for, notwithstanding the small value of the plant, and the ease with which it may be cultivated, I have been informed of some very iniquitous adulterations practised with a variety of other plants of similar colour, but possessing qualities so inert, that they must have interrupted or weakened the effects of the digitalis, by mechanically clogging its active parts in the stomach.

V. The various modes of exhibiting this medicine may, I think, have some influence upon the effects produced by it. Dr. Withering preferred an infusion of the plant to every other form, and directs one drachm of the dried leaves to be infused, for four hours, in half a pint of boiling water, adding to the strained liquor half an ounce of any spirituous water, and an ounce of this infusion to be given twice a day to an adult. If the patient be stronger than usual, or the symptoms very urgent, this dose may be given once in eight hours; and, on the contrary, in many instances, half an ounce at a time will be quite sufficient. But, without attending to this rule, I have known the digitalis prescribed in the various forms of decoction, pills made with soap, and in powder mixed with prepared chalk or magnesia; and though the two last substances may not act in any opposite way, yet they may divide the particles, and, by clogging them, prevent application to the stomach, and thus diminish its activity. A tedious decoction will dissipate the virtue of many plants less volatile than *digitalis;* but a slight one will, no doubt, injure it, and ought, therefore, never to be employed. Pills are often made up with such substances as render them soluble with difficulty. If we even suppose soap free from this objection, we, nevertheless, do not know what secret effect may be produced by it on particular stomachs, to interrupt the action of the medicine; and certainly it will not be so readily applied to that organ as in a watery form.

VI. But exclusively of the above causes of the failure of digitalis, and even supposing the proper part of the plant to be used, and in the manner directed by Dr. Withering, there is still one source, from which I am well convinced many instances of its want of success have arisen. This is, *the particular condition of the system* at the *time* of its *exhibition;* and I am disposed to think, it is the most powerful of any mentioned, though the others have doubtless contributed. It is a misfortune attending the introduction of a new medicine, that when it has been given with success

in several cases of a particular disease, it soon obtains the character of a specific, and is exhibited in every case, without discrimination or attention to the state of the complaint. The consequent failure which must necessarily follow, in some instances, is soon spread about, and the character of the remedy commonly sinks in proportion to its previous elevation. The history of many once famed, but now neglected remedies, fully proves this; and the *digitalis* has been nigh adding to the list. A bare recital of the effects of other medicines which have been much longer in vogue than the *digitalis*, and have been successful in many diseases, will show the folly of expecting uniform good effects from this or any other. I may adduce that familiar medicine, Peruvian Bark, as in point, which is well known to be so generally the most effectual cure for the intermitting fever we are acquainted with, that it has long since obtained universal credit; and yet, how frequently do we observe this famous remedy fail in that very disease, and afterwards the disease yield to other remedies, whose mode of action is directly contrary to that of the bark.* What then are the states of the system in dropsies, and other complaints in which the *digitalis* does good? In order to determine this question, let us take a view of its mode of action on the body. From the time it is taken into the stomach, and shews any operation, it is of a debilitating nature. This is evinced by its amazing power of diminishing the action of the heart and arteries in frequency and force; and by the relaxation perceived over the whole system, as well from direct effect as from the nausea induced. The confusion sometimes produced in the head, shews it possesses narcotic powers. Similar effects are produced by cicuta, stramonium, and other medicines of that class. This effect upon the arterial system has been noticed by every physician, who has given any account of the *digitalis*, and is particularly mentioned by Dr. Withering himself. Hence we should naturally imagine, it would prove useful in diseases, attended with inflammation or an excessive action in the arterial system; and, accordingly, Dr. Withering, when speaking of its use in *mania* and *hæmoptöe*, directs it to be confined to persons with "a *bounding pulse*." In such cases Dr. Ferriar, of Manchester, found the most benefit from it. I have seen it relieve a violent pain in the head, with which a lady had been long troubled, and which occasionally brought on a vertigo; the pulse was not very full but tense, and a slight degree of in-

* I allude to the cure of obstinate intermittents, especially those protracted into the winter, by bleeding; a practice I was first taught by Dr. Rush, by whom I had seen it frequently tried with success. See his Med. Inq. and Obs. vol. i. Dr. Gardiner also mentions the utility of purging in intermittents;—which is of the same debilitating tendency, though inferior in power, to bleeding. See Gardiner's Animal Economy.

creased action was evinced by other symptoms. From these facts we should be led to conclude, without hesitation, as to the particular states of diseases, in which it would be most productive of good effect, and we should totally omit it in dropsies or other complaints, which were to be removed by the cure of debility, and confine it intirely to those accompanied by increased arterial force. Some may startle at the opinion of dropsy being ever accompanied by such a state; but that it does often attend various species of watery collections, I am fully convinced, from my own experience* of the efficacy of bleeding and purging, as original remedies, or after the repeated use of tonics usually prescribed without good effect, and sometimes evident injury, and the experience of others. It will not be expected that I should digress upon this subject;—those who wish to see it fully treated, must be referred to the Medical Inquiries of Dr. Rush, whose arguments are incontrovertible. In dropsies, attended with this state only, that gentleman found benefit from digitalis; and yet Dr. Withering says, that in dropsy, it seldom succeeds in men of great strength, of tense fibre, of warm skin, of florid complexion, or in those with a tight and cordy pulse, producing the diuretic effects, in a kindly manner only in those cases where the pulse is feeble and intermitting, the

* Among many cases that have occurred, in which debilitating remedies have succeeded in removing dropsies, I will just note a remarkable one. In the summer of 1793 I attended Walter Mitchell, a waterman, for anasarca. He had been for three years previously occasionally attacked by the fever and ague, which he always cured by bark. In April, 1793, it returned in a violent manner, but it was now accompanied by a great swelling of his belly and lower limbs, and by epileptic fits; from which he was so far relieved, by repeated bleeding, as to be able to follow his usual employment. Shortly after he was exposed, in the lower part of the Jersey coast, to cold and wet, and starvation, for nigh two days, by his boat being stranded, and great fatigue in walking before a house could be found. He was again taken ill, was much swelled, had regular chills and fevers, *pulse full* and *hard*. I gave him three grains of the powder of digitalis three times a day, which produced nausea, pain in the stomach, reduction of the pulse, but not much discharge of water. I then gave of nitre and jalap, each 10 grains, every two hours, for several days, which increased the urine very considerably, and lessened the swellings. He again exposed himself; the symptoms again returned with more violence. I now bled him to 12 ounces, and repeated his powders last mentioned. A vomiting and purging came on after a few doses, and, in the course of five hours, he discharged two gallons of water: a diarrhœa succeeded for two days, and evacuated all the water. He now took bark with good effect, and would have been cured, had he not again returned to the water, which brought back his swellings. He then went to Europe.—In Dr. Donald Monro's Treatise upon Dropsy there are several cases related of blood-letting, purging, and vomiting, curing that disease; and also in the various periodical publications of Great-Britain; but it is rendered beyond a doubt by Dr. Rush, that inflammation frequently accompanies this disease. See Med. Inq. and Obs. vol. ii.

countenance pale, the lips livid, skin cold, and other symptoms of debility are present. (P. 189.)

The experience of others is directly opposed. Dr. Rush says he is satisfied, that in atonic dropsies, digitalis is not only a useless but dangerous remedy. Dr. W. Currie,* of Chester, Great-Britain, is of the same opinion. Dr. Ferriar found it to fail in several cases accompanied by the state described by Dr. Withering, as favourable for its operation. (P. 361, Med. Hist. and Reflect.) One patient was affected with gangrene in his feet, and was 70 years old. My own experience coincides with that of the two latter, having seen it do essential injury in several cases of dropsy of the atonic kind, by increasing the debility already existing, to an alarming degree. In one old man, with ascites, the pulse was reduced to 40 in a minute, and it otherwise much deranged his system. To account for the success of this remedy in dropsies, and other diseases of too much action in the arterial system, is readily done. But to reconcile the opposite testimony of Dr. Withering and others, with respect to their different success in dropsies of too little action, is very difficult: nay, to reconcile the contrary advice of Dr. Withering himself, who recommends the digitalis in mania and hæmoptöe, "with a *bounding pulse*," and in dropsies with a *weak* one, is impossible. And here I may repeat a remark I formerly made, viz. "that a medicine whose primary operation "is the reduction of the force of the arterial system, when this is so "intimately connected with the general strength of the body, should "only succeed in those cases where the latter is considerably ex-"hausted, appears not only to be paradoxical, but ill-agrees with the "uniformity observed in the operations of nature, in other parts of "the animal œconomy."† It is incumbent upon Dr. Withering to account for this apparent opposition in his own sentiments, and to satisfy the doubts of physicians, arising from the action of the remedy being directly contrary to the principles upon which alone certain dropsies can be cured. I may also remark, that it is much to be wished physicians were more accurate in noting the state of their patients systems, at the time of prescribing this and other remedies; and also the force of the pulse at the same time, with its frequency; which, though so constantly stated, is of far less consequence in determining the situation of the system, as to the important point of the existence or absence of inflammatory disposition, than the former. We should thus arrive, with more certainty, at the true virtues of a medicine, and be able to reconcile the contradictory accounts of physicians respecting its utility in the same disease.

* Mem. Med. Soc. Lond. Vol. iv. p. 11.

† Obs. on the Weather and Dis. for Oct. 1791. See American Museum.

The inattention of physicians in the exhibition of *digitalis*, to the rules prescribed by Dr. Withering, has not escaped his notice, as appears by the following extract of a letter from him to Dr. Woodville, and published in the "Medical Botany" of the latter. "Under my own management, under that of the medical practitioners in this part of England, and, I may add also, in the hands of some worthy and respectable clergymen in village situations, it continues to be the most certain, and least offensive diuretic we know, in such cases, and in such constitutions as I have advised its exhibition. I have also the satisfaction to find, by letters from some of the most eminent physicians in different parts of England, that it is equally useful and safe in their hands. But I complain of the treatment this medicine has had in London. Its ill-success there cannot be altogether owing to difference of constitutions. Dr. Lettsom* has related his unsuccessful attempts with a degree of courage and of candour, which do the highest honour to his integrity; but no one can compare *his* choice of patients with *my* declarations of the fit and the unfit, or the doses prescribed, and the perseverance he enjoined, with my rules and cautions, without being astonished that he could suppose he had been giving this medicine 'in the manner prescribed by me.' I am fully satisfied that had I prescribed it in such cases, such forms, such doses, and such repetitions as he has done, the effects would, in *my* hands, have been equally useless, and equally deleterious; I must, therefore, suppose, that he had forgotten what I had written, without being conscious that his memory had deceived him. Had it been otherwise, after perusing the cases I had published, at p. xx. and 51, of my account, he would hardly have thought it necessary to have published more instances of what I had stigmatized as bad practice, or to have sought for further proofs, that an active and useful medicine might be employed, *so as to prove* deleterious poison."

It may not be improper to add some cautions and observations upon the use of digitalis, not mentioned in the above paper, for the information of those who may not have seen Dr. Withering's work, and may wish to exhibit the medicine.

The Doctor desires the medicine to be given in the doses, and at the intervals mentioned above, and to be continued until it either acts on the kidneys or stomach, pulse or bowels: to be stopped upon the first appearance of any of these effects; the patient to drink very freely during its operation.

In case of *ascites* and *anasarca*, when the patient is weak, and the evacuation of the water rapid, the use of a proper bandage is

* Memoirs Med. Soc. Lond. Vol. ii.

indispensibly necessary. If the water should not be wholly evacuated, it is best to allow an interval of several days before the medicine be repeated, in order that food and tonics may be exhibited.

The flow of urine will often precede, sometimes accompany, and frequently follow, the sickness at stomach, at the distance of some days, and not unfrequently be checked by it, especially if given in too large doses. The sickness will sometimes cease, and recur again as violent as before, and will continue to recur for three or four days at more distant intervals. These sufferings are not necessary, and are no objection to the use of *digitalis*, neither is a stone in the bladder.

ARTICLE II.

OBSERVATIONS UPON THE BAD EFFECTS SOMETIMES PRODUCED BY EATING PHEASANTS.*

By JAMES MEASE, *M. D. Resident Physician of the Port of Philadelphia.*

IN the month of February, 1791, several persons in Philadelphia were seized, in about three hours after dining upon pheasants, with giddiness, violent flushing of heat and cold in the face and head, sickness at stomach, and repeated vomiting. These symptoms were soon succeeded by delirium, weak pulse, and the greatest debility, which threatened approaching death. Some cases were marked by the preservation of the senses, but a total inability to articulate. By plentiful draughts of warm water, or chamomile tea, the vomiting was promoted, and the offensive matters discharged. Gentle stimulants and cordials, as wine whey, volatile salts, and liquid laudanum, were afterwards exhibited, and most commonly with success. In some cases, however, the event was not so fortunate; from the co-operation of other causes, the attack was more violent, and either death, or a protracted recovery, ensued.

The late George Bryan, one of the judges of the supreme court of Pennsylvania, dined, with Mrs. Bryan, upon roasted pheasants; and after walking out two squares, called at the house of a friend, where he drank a draught of cyder. He was soon afterwards informed of the sudden indisposition of Mrs. Bryan, and ran home in great haste. Being upwards of 60 years old, short, and of a clumsy built, the exertion to his system was immense; and he had no sooner arrived home, and beheld Mrs. Bryan in the alarming situation described, than he fell upon the floor, apparently in an apoplectic state, which, in the course of a few days, proved fatal. Mrs. Bryan, after being very ill, recovered.

The same day, an old lady, Mrs. G. also dined upon roasted pheasant; her negro woman also, advanced in life, eat part of the same bird, and as in the case of Mr. and Mrs. Bryan, no other animal food was eaten. In about four hours after I was called to their assistance, and found them both labouring under the above symptoms: both recovered by plentiful dilution and gentle cordials.

* *Tetrao Cupido*, LIN. This is the *partridge* of New-England.

Another instance of the bad effects produced by eating those birds fell under my notice in April, 1792, in a gentleman aged 62; who had, for a long time, been subject to the gout; but, for some months before, was particularly affected with it in his breast, causing the troublesome symptom, *Angina Pectoris*. Having somewhat recovered from the violence of the disease, he was tempted to gratify a returning appetite, by dining upon a pheasant. His meal consisted almost intirely of the back, except a small proportion of bread. In about four hours after, while sitting in his arm chair, he was seized with a faultering voice, and in a few minutes was intirely deprived of the power of utterance, though he was sensible of his situation, and had a great inclination to speak. A gentleman in company with him at the time, perceiving his alarming situation, dispatched a messenger for Dr. Rush, his family physician, who arrived in a short time, and found him apparently in the agony of approaching dissolution. His pulse could scarcely be felt: he, nevertheless, took about 12 ounces of blood from his arm immediately, and with the most sensible good effects: for, during the flowing of the blood, he exclaimed, "I am better." An emetic was afterwards administered, and I arrived during its operation. He discharged the remains of his meal, which by reason of the slow and feeble action of the stomach was but half digested. An anodyne was afterwards exhibited, which produced a calm, refreshing sleep: a blister was also applied to the breast to relieve the difficulty of breathing, under which he laboured from his old complaint, and was now rendered more severe by the irritation of his meal. During the night he slept tolerably well; but I was frequently alarmed by interruptions in his breathing, and by the irregularity and interruption of his pulse. He soon recovered from the effects of the pheasant; but his old complaint harrassed him to the last.

These, and several similar cases, are the foundation of my opinion of the occasional poisonous quality of pheasants; and, in order to account for their bad effects, I would observe, that in the winter, and early in the spring, these birds live chiefly upon buds of trees, and particularly upon those of the laurel,* which is one of the few shrubs that preserves its verdure throughout the winter, and, from many melancholy proofs, is known to be highly poisonous. It is not improbable, therefore, that the noxious quality of the laurel may have been communicated sufficiently strong to the pheasants to produce the effects described. Some of the faculty, who heard of the above cases, accounted for the symptoms, by supposing that the juice of the laurel, extracted by the bird, had been mixed with the sauce from the carelessness of the cook in opening

* *Kalmia latifolia*, LIN. Sometimes called the *shrub ivy*.

the craw; but this could not be the case in the last instance, for the craw of the bird was carefully examined after the accident, with a view of ascertaining the fact, and was found intire, with its contents preserved. I do not know whether the craws of the pheasants upon which the persons dined who are mentioned above, were also examined, but the single fact is sufficient to prove that the opinion advanced is false. Neither do I apprehend any necessity exists, to have recourse to the supposition, to account for the bad effects of these birds. We have a number of similar facts to prove, that not only the flavour, but the quality of certain plants, and even minerals, may be communicated to the flesh of different animals feeding upon them. Fishes, it is well known in the West-Indies, are sometimes highly poisonous, although in the taste or smell there may be nothing indicative of a noxious quality; and so powerful is the poison, that a gentleman informed me he has seen a person at dinner fall off his chair and expire, in St. Domingo, two minutes after having swallowed a morsel of a yellow sprat.* Hence the common practice of putting a silver spoon or coin into the pot in which the fish is boiled, that a judgment may be formed of the safety or danger of eating them by the permanency or change of colour in the silver. The London mutton is known to taste strong of turnips, with which the sheep prepared for market are chiefly fed. Dr. Rush informed me, that when at the college of Princeton, almost the whole of the students were severely purged by dining on wild pigeons, which had fed upon the berries of phytolacca, or poke.

But it has actually been asked, if the bad effects observed from the pheasants are produced by the impregnation of their flesh with the laurel, why does it not also poison these birds? This question can easily be answered by referring the fact to a law which appears to be established through the scale of animal creation, by which certain articles of food are intended for the support of particular animals, while, to others, they may be highly injurious. Every one acquainted in the least with natural history, will recollect instances of the truth of this. The example of the fish will be immediately referred to as direct; and, I may add, the horse is killed by eating hemlock, which affords a very agreeable nourishment to the hog: although, therefore, the laurel produced no bad effect upon the pheasants, yet the instances above related are decided proofs of the bad consequences from eating these birds. The same

* The poisonous effects of these fishes are attributed to their feeding upon copper banks, in the West-Indies. Dr. Goldsmith (Animated Nat. Art. fish) notices the fact, and this theory; but disbelieves it, as, he says, he knows of no copper mines in America. Had he lived, however, a few years longer, he would have found that they abounded in America, and were actually worked at the time he declares his ignorance of their existence.

peculiarity, with respect to certain articles of diet, prevails among men. Many substances are deemed delicious by some, which, to others, are both disagreeable and hurtful: the almond, for instance, is commonly esteemed a pleasant fruit, but there are some who will be thrown into fever, attended by universal eruption, by eating a single one not deprived of its husk.

It is true that pheasants are often eaten with impunity, and I never eat more of them than during the very season when the above instances occurred of their bad effects. But I imagine that the circumstances under which they are commonly used, tend to diminish their hurtful properties: For, 1. From their great delicacy, they, in general, form only a part of our meals, and are mixed with a large proportion of other articles of food, previously taken in, and which constitute the solid part of our repast. The part of the pheasant eaten is thereby involved in such a manner as prevents the direct application of it to the coats of the stomach, and, consequently, from producing the bad effects that followed their use in the above cases. The wine and other stimulating and diluting liquors taken in, tend likewise to counteract and weaken the effects that might otherwise be produced; but in the cases I have related, nothing else was eaten except a trifling proportion of bread, and nothing drank but a glass of small beer: the noxious quality of these birds had thus a greater opportunity of being exerted, by being directly applied to the stomach, and having nothing to counteract it.

2. Another circumstance tending to increase the noxious quality of the pheasants, was the lateness of the season in which they were eaten, when the laurel had begun to vegetate, and, consequently, to acquire a greater degree of activity than during the intensity of the winter, when its powers are weakened by the concentration of the sap in the root. This cause operated particularly in the last case.

These facts are fully sufficient to shew, that though the pheasant has commonly been esteemed an harmless bird, they ought to be eaten with caution; and suggest the propriety of a rule in prescribing them as an article of diet: for, as they are generally esteemed a great delicacy, and very light food, they may be wished for by convalescents; and, as their stomachs are in too highly excitable state to take wine or any other food to counteract their bad quality, it will be right to debar them from these birds, and to substitute in their place other articles of a less suspicious nature, and of easy digestion, as rabbit, partridge, or chicken.

ARTICLE III.

OBSERVATIONS UPON THE YELLOW FEVER, AND ITS PROXIMATE CAUSE.

In a Letter from Dr. GEORGE DAVIDSON, *dated Fort-Royal, Martinique, Sept.* 20, 1796, *to* JAMES MEASE, *M. D. Resident Physician of the Port of Philadelphia.*

THE Yellow Fever has again made its appearance here, in a more violent degree than I ever recollect seeing it. The rainy season, which, in common, sets in about the first week in May, did not make its appearance until the month of July. June proved a very agreeable month; towards the latter end we had remarkable sultry weather. The thermometer, in the shade, stood for several days at 90 deg. at two P. M. This proved a prelude to the appearance of the disease. I have no doubt when the mercury stands at 86 deg. or above it, for several days, the weather being at the same time calm, that exhalations from swamps, and miasmata from putrid vegetable and animal matter, chiefly are capable of producing the disease; that, at the same time, it may be propagated by contagion, the heat and putrefaction still operating: accordingly I found, that in several of the ordnance ships, which had arrived from St. Vincent and Grenada, after the partial reduction of those islands, having the disease on board, soon propagated it to the other ships, along side of which they were laid up during the hurricane months in the Carenage, a secure but unhealthy part of the bay of Fort-Royal; surrounded on every side by swamps, and a nasty beach, rendered still more so by the want of a certain conveniency unkr wn in a French island. The disease spread rapidly during the months of July and August, and was only checked by the plentiful rains which fell towards the latter end of August and beginning of September.

To the history of the disease I have little new to add. The symptoms at the beginning were more violent and more distressing than in former years. The pains were not confined to the head, loins and calves of the legs, but were general, particularly from the knees to the lower extremities, and from the elbows to the wrists, for several hours preceding the attack. A pain, confined to the forcheads and socketts of the eyes, dimness of sight, faintness and syncope, generally ushered in the attack: these were soon followed by a burning heat over the whole body, sickness at stomach, pain under the right breast, shifting to the left, anxiety and

uneasiness in every posture. The belly was in general costive; the tongue, throughout the whole disease, generally much covered with a slight brown scurf. In some fatal cases I have observed a singular appearance, a jaggedness towards the edges, and a deep furrow in the centre, the tongue at the same time moist. The pulse was seldom full in the commencement, but irregular, and frequently intermitting; in many cases and those the most dangerous, it did not exceed in frequency nor strength the natural state. The urine was high coloured and spare, and sometimes a suppression took place. Bloody urine was a fatal symptom. Deafness, when it happened early, was always a fatal symptom, but, toward the end, favourable. It is worthy of remark, that no bile is ever thrown up or down. The biliary ducts appear to be spasmodically affected.

I have met with several cases of bubos, so called, both in the groin and arm-pits. Anthraces and carbuncles I have seen in four or five cases. The carpenter of the ship John, of Philadelphia, had one on his elbow, which discharged several quarts of blood, under which he sunk: two more appeared upon the ancle and instep of the same foot: the hæmorrhage could only be restrained by the application of pledgits of vitriolic acid and water.

Several of my medical friends have prosecuted dissections at the hospital: from these, and my own observations, I am clearly of opinion, that the brain is the principal organ affected, and that the other affections are merely symptomatic. Wherever other appearances were met with, they might perhaps be traced to some previous labes of the constitution, which determined the disease to exert its effects upon that weakened organ.

I am only sorry to observe, that this disease often attacked with such violence as baffled every attempt. The inflammatory stage was but of short duration, and the symptoms of exhausted nervous energy so soon appeared, that we were afraid to use the lancet. The few cases in which I tried it, did not succeed. Of the cold bath used in the early stage of the disease, before the constitution was materially affected, or a particular determination had taken place, I have a high opinion. I have mentioned the circumstances in which I found it useful to Dr. Rush in 1794. I have lately tried it in a protracted case with great success. The calomel, though it sometimes failed, upon the whole, succeeds best. I generally gave in the beginning the solution of *vitriolated magnesia*, joined to half a grain tartarized antimony, taken *par reprise*. It seldom vomited above thrice, and purged the patient plentifully. There was generally a torpor of the lymphatic system, which prevented the action of the mercury. We were obliged to give it in large doses before a salivation could be exerted. Dr. Chisholm, the ingenious author of the treatise upon the Yellow Fever of Gre-

nada, now surgeon-general of the ordnance, has been with us here some weeks, and we have attended several patients together. At his request I have tried the mercurius calcinatus, in doses of two grains, joined to one of opium. It has succeeded in three cases within ten hours, in one case a salivation was excited, and the patient was out of danger, as I have in every case observed. Purging, though it relieved in the beginning, did not contribute to the cure: the mercury seemed to act by exciting a different action intirely, and I was obliged to add opium, to prevent it going off by the bowels. Vomiting, and the train of symptoms connected with it, was best relieved by the vitriolic æther, blistering the stomach and purging. I was told of one case of recovery at St. Lucia, from the black vomit, by the patient drinking a quantity of sallad oil and pickle. The head-ach was relieved frequently by applying cloths wet in the solution of *muriated ammonia* to the head previously shaved. It excited the mercurial action upon the salivary glands in one case very suddenly.

I have already mentioned my experiments* upon the composition of atmospheric air, which I have frequently repeated in presence of the late Dr. Charles Webster, of Edinburgh, Dr. Saunderson, of London, and Dr. Chisholm. I found an absorption of from 67 to 70 parts in the 100: a proportion of oxygenous gas more than double what is observed in Europe. It may, perhaps, serve to explain the proximate cause of fever in general, and of this in particular. Permit me to give you my sentiments upon the subject. I have to apologize for the haste in which I write, and the want of the ornaments of composition to render the subject

* Thefe were communicated in a former letter, dated Aug. 22, 1795. "May 23. lat. 34 deg. 30 m. long. 64 deg. wind W. moderate, fair; therm. 69 "at noon: one meafure of nitrous air, and one of atmofpheric; there remained 1. 54.—May 27, lat. 29 deg. 50 m. long. 60 deg. wind N. N. E. "moderate, clear and fair; therm. 74. mixture of nitrous and atmofpheric "airs 2, remained 1. 50, or half abforbed.—Lat. 23 deg. long. 58 deg. therm. "78 fame. June 20, Trinidad; lat. 10 deg. 20 m. long. 62 deg. one meafure of nitrous and one of atmofpheric air mixed, left only 1. 34. There "was of courfe 66 abforbed. I was aftonifhed to find this refult. The "thermometer was at 87, or nearly fo, three different days on which this "experiment was made. Not having Dr. Prieftley's obf. with me I cannot "fay what proportion of nitrogene gas itfelf might have been abforbed: "the abforption, however, was inftantaneous: the water had not time to "act upon the gafes. This much is certain, that we have it now in our "power to account for the ruſting of metals, fermentation and putrefaction, from the greater proportion of oxygene in the air. The production "of hydrogene gas from the diffolution and putrefaction of animal and vegetable fubftances, no doubt is chiefly accelerated by heat and moifture, "and heat alfo enables the atmofpheric air to fufpend the muriate of foda "taken up by evaporation from the fea, which has alfo its fhare in the production of ruft."

more interesting. We must premise, however, that the whole is nothing more than a probable guess; but I may lead others to investigate the subject, who are possessed of more abilities and leisure.

A question here occurs, why is the Yellow Fever so fatal to Europeans just arrived, or people from cold climates; those of a robust habit of body and prime of life? Dr. Rush, in a conversation with me, supposed the blood of Europeans, newly arrived, was more highly oxygenated. I will rather make another supposition. Europeans, newly arrived from cold climates, have a firmer texture of fibres, cold tending to brace and invigorate the body. Exercise in cold air braces and constringes the surface, and remarkably increases the appetite and digestive powers; beside this connection between the stomach and surface, a remarkable connection subsists between the stomach and lungs. Whatever tends to brace the former increases the action of the lungs, a greater quantity of atmospheric air is taken in, and a greater proportion of oxygene is fixed; hence the vigour infused over the whole system; and hence the vigour and energy that so suddenly follows a full meal upon animal food, and a few glasses of generous wine. As the atmospheric air of Europe contains but a small proportion, comparatively, of oxygene gas, the quantity taken in and fixed bears some proportion to the necessities of the system, and the balance is preserved; but in this climate, while the powers of vigorous digestion, and the proportional action of the lungs still remain unimpaired, a much greater quantity of oxygene is inspired and fixed in the blood, the irritability will be increased, and the system hyperoxygenated; hence the predisposition to fever is formed. In this climate, with Creoles, or those long settled in the country, debilitated by residence under a vertical sun, the powers of digestion, and the necessary connection between the stomach and lungs, prevent a greater assimilation of oxygene than is consistent with health; the pores of the skin are pervious, and the caloric generated is carried off by perspiration; possessed of a less firm texture of fibres than Europeans, they are not so much disposed to suffer from disease. No disease, however, from this predisposition will follow, unless some exciting cause combines, as the system will gradually accommodate itself to climate; the habit, enervated by constant heat, will no longer preserve its vigour, and no more of the oxygenous principle will be fixed than what is consistent with health, or the wants of the œconomy.

It is possible, in this climate, to preserve a degree of health unknown in the variable climates of North-America and Europe; and, accordingly, we find in the healthy islands of St. Kitts, St. Vincents, and Barbadoes, soldiers arrived from Europe, have remained there for years, and enjoyed a degree of health unknown

in any other part of the world, notwithstanding their debaucheries. Wherever the barracks are in an airy situation, and not in the neighbourhood of swamps, and the soil of the country around is loose and sandy, permitting the rain, as it falls, to percolate through, the air is cooled by plentiful rains, and is rendered more grateful and refreshing; it is only in situations where water stagnates, and vegetable putrefaction goes on, and hydrogenous gas is extricated, that the disease in question arises. I formerly remarked to Dr. Rush, before I had formed any Hypothesis upon the predisposing and exciting causes of fevers, that upon Berkshire-hill, St. Vincents, the situation of the fort and barracks was to all appearance healthy, being a high ridge, surrounded mostly by the sea, and without any neighbouring swamps; but experience convinced us to the contrary; for part of the 66th regiment removed from their healthy situation upon an opposite hill, about half a mile distance, lost more of their men in a few months than the whole regiment had lost in as many years. It is necessary to remark, that this hill or ridge ended in a high peak, which it was necessary to dig down 60 feet, to give a broad base to construct the necessary buildings for defence; and two different volcanic strata, lava, pyrites, &c. separated by loam and vegetable earth, were cut through; the rain falling upon this, was probably decomposed, and hydrogene or hepatic gas afforded. I mentioned another singular circumstance to Dr. Rush, respecting the 6th regiment at Barbadoes, which was stationed at Kings-hill, a short distance from town, at the end of 1793, during the prevalence of the Yellow Fever; there was a pond of brackish water close to the barracks, and after the fever had ceased sometime, it was observed that the men were apt to be seized afresh after slight showers; the pond was covered with green moss or conservæ, and it struck me, that the rain, in falling, broke the crust, and allowed the noxious vapour to escape.

Upon the whole, therefore, I conceive that hydrogene gas is the exciting or proximate cause of fever, modified possibly by numberless external and accidental causes; and, in particular, by the state of the patient, producing the variety with which we meet; as intermittents, bilious and nervous remittents, dysenteries, &c. Wherever there is a labes in the constitution, the disease, like a treacherous enemy, attacks the weakest part; and even in the progress of the disease, if one part has suffered more than the rest, it concentrates its whole force upon that part. Hence, where there has been a retention of acrid fæces, and the gazeous oxyd of azote formed, a constriction of the surface of the body by cold or wet will produce dysentery; and, where the lungs are previously weakened, the disease will attack that organ, especially if the constitution of the air favours the attack.

I will proceed a step still further in supposition. I conjecture

that the hydrogenous and nervous gas, or fluid, are the same: possibly we may hereafter find that all the phenomena of electricity may depend upon the same principle; that the accumulation of hydrogene gas in the constitution may, in certain conditions, produce an excitement of a pleasant nature, is probable from the remarkable flow of spirits and hilarity of temper, which sometimes usher in the attack of the Yellow Fever. I suppose, also, that, like the electrical fluid, it may even be accumulated in the system without exerting its effects, until called into action by some exciting cause, whether that be contagion, the gazeous oxyd of azote, the sudden application of cold to the body; passions of the mind; bodily or mental fatigue. I suppose the effects of its accumulation, when called into action, are principally exerted upon the system of the brain and nerves; and that it is capable, by the continuance of its action, to produce in that system a state of exhaustion of the nervous influence; and a waste of oxygene. Hence, a deficiency of irritability, speedy putrefaction from that cause, dissolution of the fluids and death: hence those diseases where the nervous fluid is suddenly pumped out, terminate soonest in putrefaction; for instance, hydrophobia, tetanus, and the disease in question. Hence we might also expect to find in bodies interred, the conversion into fat from their being deprived of oxygene; hence also we may account for the sudden state of obesity after recovering from this fever.

To preserve bodies from sudden putrefaction, nothing more would appear necessary than to prevent the waste of the nervous fluid; hence the practice among fishermen of knocking on the head those salmon which they wish to crimp.

I have read Dr. Mitchill's ingenious essay upon Gazeous Oxyd of Azote, and would allow, that it has a great share, as a powerful exciting cause, in rendering the predisponent and proximate causes of fever more active; but to its being the principal agent I have strong objections. From the observations of Dr. Rush it appeared, that butchers, and those that lived in the neighbourhood of shambles, scavengers, grave-diggers, and others of similar employments, escaped the Yellow Fever in Philadelphia;* I should suppose that those people would have been more exposed to the gazeous oxyd of azote than any other class. Among sailors fed upon putrescent animal food during long voyages, we may suppose a great extrication of this air takes place; and yet fever is never produced; but a disease of an opposite tendency, viz. scurvy, marked by torpor, and a want of excitement. From all the observations I have been able to make, or to collect from the writings of authors upon this subject, it strikes me that vegetable putrefaction has a greater share in producing the disease than animal. It is well

* See Dr. Rush's Account of the Yellow Fever, pp. 101, 102.

known, that in the putrefaction of animal substances, a much greater proportion of the gazeous oxyd of azote is extricated than in vegetable putrefaction; was Dr. Mitchill's hypothesis well founded, the consequences and deductions would follow contrary to our observation.

There has been much difference of opinion among physicians respecting the contagious nature of the Yellow Fever. Some deny it altogether, and others assert that it is, in almost every instance, propagated by contagion. I must declare I have known the disease evidently propagated in this way; but, in many instances it could not be traced: I have known three cases of the fever brought on by persons bathing in the sea along side the vessel, some distance from the shore; and neglecting to dry themselves properly afterwards. The seminia of the disease were here present, and like the electrical jar charged, required only the approach of a conductor. Upon this view of the proximate cause, the plan of prevention may be easily understood, by a spare and vegetable diet, to diminish the tone and vigour of the system, and of the stomach and lungs in particular, thereby preventing the accumulation of oxygene in the system, and avoiding all occasional and debilitating causes, especially during the prevalence of the epidemic; as debauchery, night air, and exposure to the heat of the sun; by removing from the vicinity of swamps; by an open belly; and, lastly, by exchanging a low for a high situation, where the atmospheric air has less oxygene, and a greater proportion of azote. The use of the warm bath may prove salutary by opening the pores of the skin. Much might be done in preventing the accumulation of the oxygenous principle, by such articles of regimen or medicines as would fix it. The good effects of carbone, as recommended by my fellow student Dr. Odier, of Geneva, in active hæmorrhages, were probably owing to its fixing oxygene, and depriving the vascular system of its irritability.

Are we to look for the salutary effects of mercury in its taking off the irritability of the system, and preventing the expenditure (if I may say so) of the nervous fluid? or in its taking off the determination to the brain, removing the *engorgement* of that viscus, and preventing effusion, by exciting a new action of the system?

I have, in general, remarked, that while elevated situations proved health and salutary to the whites, who had removed there for the benefit of a change of air, it was far otherwise with the negroes, most of whom, in that situation, laboured under diseases of the stomach; the Cachexia Africana, or Mal d'Estomac, a disease arising from weaker excitement, or deficiency of oxygene in the blood. This is a proof, among many others, that the system, worn out by fatigue, low living, and violent, long-continued heat, requires a purer air, surcharged with oxygene, to produce the necessary excitement conducive to health.

ARTICLE IV.

CASE *of an* EXTRAORDINARY DISEASE, *in a* CHILD, *apparently* SCROPHULOUS.

By PHINEAS HEDGES, *Physician at Newburgh.*

IN October, 1791, I was called to a child, aged five years, whose mother, previous to, or during her pregnancy with him, had been afflicted with a cough, and other symptoms of a phthisis pulmonalis.

She, however, so far recovered after her delivery of this child, as to conceive again, of which also she was delivered. But shortly after her phthisical complaints increased with violence, and put an end to her existence in the year 1791. The child of which she was last delivered, in consequence of her death, was put to a dry nurse, under whose care it lingered until it was nine months old. Previous to its death, large scrophulous tumors arose on each side of its neck, of a very hard and unusual consistence: but what more immediately contributed to terminate its existence, was the pertussis, or hooping-cough, which, at that time, was very rife in the country. Although emaciated to an extreme degree, it had, at all times, a most vigorous (or rather ravenous) appetite. The pertussis appeared rather to be an auxiliary than the proximate cause of its death. Its constitution was radically infirm; and its infirmities were of such a nature as to leave no doubt that they proceeded from the mother. The emaciation was an evidence that the glands of the mesentery, as well as those of the neck, were tumified so as to prevent the absorption of the chyle by the lacteals. I am convinced, that both this child, and the one who will be the subject of this paper, inherited a scrophulous disposition from their mother. We might here inquire, first, how far the present notions of conception coincide with the notion of hereditary diseases. Secondly, how far the present doctrine of conception agrees with the pathology of the solids. If (as it is asserted) man exists in miniature in the male semen, complete in all his parts, how can a disease of the mother affect the child, except by vitiating the fluids of the fœtus, and thereby rendering them unfit to increase and support its solids? But if the placenta is a secreting or absorbing organ, secreting or absorbing the more fine and healthy part of the blood, such a consequence is not to be expected. It is asserted, that the fœtus of a mother, labouring under the venereal disease, is never contaminated with that complaint, except it come (as it must) into contact with the diseased parts of the mother in time of

delivery. I shall leave these points to be settled by speculative inquiries, and pass on to the subject of this narrative.—The child to whom I was called had been observed suddenly to halt in his gait, his abdomen to enlarge, and his spine to become crooked. At first I supposed it to be a rheumatic complaint, originating from a strain, or some accidental injury.

I, therefore, ordered a cathartic and an emplastrum roborans to his spine, expecting, in a few days, the disorder would vanish.

Shortly after I called again, and found his complaints not in the least abated. As a considerable tumefaction had taken place in his abdomen, and as his age indicated, I justly supposed him troubled with worms. To clear the way for the operation of medicines, upon his original complaint, I thought it advisable to empty his bowels of their extraneous, unnatural contents.

I therefore exhibited calomel and dianthus very successfully; after the operation of which I directed the use of iron. When under this mode of treatment, I ordered his spine to be frequently rubbed with a strong tincture of cantharides. Finding no material benefit from this procedure, I directed the part to be rubbed with spts. tereb. and to take from two to ten drops twice a day. Soon finding the inefficacy of these means, I applied to his loins a large epispastic, and ordered it to be re-applied as soon as the discharge should cease. The discharge was kept up for two or three weeks. Although the incurvation of the spine seemed, in some measure, to lessen, under the operation of epispastics, I was confident they would not be sufficiently powerful to remove a disorder of such an obstinate nature.

I therefore resorted to the exhibition of the tincture of guiacum; and, as I recollected the encomiums passed upon cream of tartar by Dr. Arbuthnot, in rheumatic cases, and as I still supposed it a rheumatic affection, I presumed the exhibition of these two medicines, in conjunction, would render them more efficacious. Accordingly I kept him under the constant use of these remedies for a month, increasing the quantity from day to day, as the system became habituated to their use. The incurvation of the spine lessenned, the tumefaction of the abdomen subsided, and appearances flattered a complete cure. But shortly, all these favourable symptoms vanished, and he was soon hurried into the same condition in which he began the use of these remedies. The subsidence of the abdomen I imputed chiefly to the use of the cream of tartar, because, it has been observed, that all saline substances have that effect upon the swollen bellies of children. They produce this effect, I presume, by dissolving the tenacious mucus which often lines the primæ viæ of children, and to which the tumefaction of their bellies may often be justly imputed.

In this instance it might have been the soluble effect of both

these substances conjoined, and no ways peculiar to the operation of the cream of tartar. Notwithstanding the retrograde appearance of the symptoms, I had determined to continue their use in a more enlarged quantity. But previous to my laying them aside, an indolent swelling had arisen in the most fleshy part of his thigh, and, at that time, began to be considerably inflamed. A suspicion arose in my mind, that the stimulant power of the guiacum would increase the pain and inflammation, especially as the swelling was large, and a considerable degree of fever had already invaded the system. The tumor had that albuminous, springy, elastic feel to the touch, which Mr. Bell considers as peculiarly characteristic of white swellings of the joints. In the appearance of this tumor I readily discovered the retrograde state of the symptoms. Being now convinced that his complaints originated from a general scrophulous diathesis, derived from his mother, I laid aside the use of all medicine, except a little volatile liniment, which I ordered to be rubbed on the tumor to amuse his friends. I, therefore, waited until suppuration took place, hoping a discharge might relieve the incurvation of his spine.

In opening the abscess, a thin, serous matter, mingled with a white curdled substance, was discharged, to the quantity of a pint. Over the orifice I applied a plaister of diachylon (as no other dressings could be confined to the part.) The wound soon put on the appearance of all scrophulous ulcers, and the incurvation of the spine rather increased than lessened. When I found the discharge did not remove his original complaint, and, I presumed, the inflammation of the tumor had subsided, I resolved to make a second attempt with the guiacum and cream of tartar. I therefore began with the quantity I had left off with, and administered it from day to day, for a considerable length of time. By its use, the belly again subsided, the crookedness of the spine seemed to diminish, although not in that degree it did under the first use of these remedies. I, however, persevered until I was fully convinced no permanent good effect could be expected, for the tumefaction of the abdomen, and the crookedness of the spine, from some hidden cause, had become as bad as when I last began the use of these remedies. I was now resolved to give him up to Nature, as I was sure his complaints were too obstinate for the present imperfect state of the healing art.

He, therefore, continued without any medical assistance, much in the situation I left him, until the winter of 1795, at which time he was seized with the hooping cough. In the interim, between the time I dismissed him and his taking the cough, a number of tumors arose, which suppurated, and were opened. From the time his first tumor arose his pulse beat with great velocity, so quick, that had I not been an eye-witness, I must have presumed,

a circulation so rapid would have worn out any constitution, and especially his, enfeebled by perpetual disease.

But, at all times, and in all circumstances, his appetite was vigorous, and, in most cases, ravenous. When attacked with the hooping-cough, he had a most violent fever, together with a discharge of blood from his lungs. The colour dark, the consistence thin, and the discharge without any mixture of phlegm. I was then confident the hæmoptysis proceeded from a rupture of blood-vessels, and that his existence would be soon terminated, either by the then raging fever, or by a succeeding phthisis pulmonalis. Neither of these events happened: for, by the assistance of epispastics and lac ammoniac, or rather by the efforts of nature, he so far recovered as to limp about as he had done before he was seized with that complaint. The evacuation of blood from his lungs must not have proceeded (as I supposed) from a rupture of vessels: for, if a rupture had taken place, we could not reasonably have presumed it to have healed when all the ulcers on the surface shewed a disposition to keep open, in spite of the means used to heal them. To the want of tension in the vessels, to a diminution of their resisting energy, and the consequent tenuity of the blood, the hæmorrhage must be referred. From the time the hooping-cough left him, until the summer of 1796, he remained in a situation very similar to what he commonly had been from the first appearance of his ulcers, his pulse rapid, and his appetite vigorous. In the beginning of the summer 1796, another large tumor appeared, near two inches distant, on the left side of his spine, about in a range with the second dorsal vertebra. It continued very painful until it suppurated, and was opened; it discharged a fluid of the same appearance as the one first mentioned. The curdly appearance, I suspect, may be imputed to a portion of the coagulable lymph effused with the serosity, or to the effusion and coagulation of the lymph, strictly so called. I shall not here enter into the investigation of this point, as it will be more properly left to the systematic.—Another tumor, soon after the opening of the last, appeared, which suppurated, and was also opened. Since that period his pulse has been very quick, his appetite still vigorous, and what is more remarkable, he voids part of his excrements from the orifice of the ulcer on his back, some from that on his abdomen, and some from the natural passage.—In the season of fruits, their seeds were often observed, in dressing his ulcers, to issue from their orifices.

And lately a number of worms have been discharged from the ulcer situate on his abdomen. A bystander will often hear wind discharged from the ulcer on his back. I introduced a probe in the ulcer of his back about one inch, in a perpendicular direction, and in that of his abdomen, two inches or better, in a slanting

direction, and I am well convinced, that the probe entered the cavity of the intestines, both from the orifice of the ulcer on his back and abdomen.

When the probe reached the spot which bounded its progress, I could sensibly perceive that the part gave way, in a manner that would naturally be expected from the thinness and looseness of the intestines. I was not able to introduce the probe in any direction so as to follow the course of the intestinal cavity. To account for this phenomenon, we must have recourse to an attachment of the intestines to the inside of the abdominal cavity. During the inflammatory state of the tumors an adhesion must have formed, and when suppuration had taken place, the retention of the discharge has given occasion to a perforation of the intestinal cavity. The pus now discharged is of the same colour and consistence with the discharge of every scrophulous ulcer. The lips of the ulcers have the same contracted, shrinking appearance; the circumjacent parts have the same livid colour; and every circumstance pertaining to his complaint evinces it to be genuinely scrophulous. To sustain such accumulated afflictions evidences a very great inherent power in the constitution: for any constitution, however robust, must give way to the following grievous symptoms, to wit, an enlarged abdomen, an incurvation, a recurvation of the spine, a perpetual discharge and a perpetual fever, except it be supported by some inherent power. Four only of the vertebræ appear to be affected. There are two prominences, one formed by the first lumbar, and the other by the tenth dorsal vertebra, both inclusive making four vertebræ. A crooked spine in infants has generally been connected by writers with a debility of the lower extremities. But the lower extremities of this patient are entirely unaffected. The incurvation of his spine seemed at first to constitute his only complaint. The crookedness of his spine is, however, most certainly to be referred to a scrophulous diathesis. It has been supposed by Mr. Pott, that the ligaments interposed between the different vertebræ of the spine were eroded, thereby rendering the motion of the spine stiff and crooked. How a scrophulous habit should produce this effect I am at a loss to determine, and still more to determine how an issue or seton should restore the eroded ligaments to their pristine state and vigour. If I was permitted to oppose conjecture to anatomical fact, (if such a fact is ascertained by dissection) I should rather impute it to affections of the ligaments, unattended with any erosion or destruction of their substance. It is true, that a loss of bone and flesh is repaired by a sort of new growth, calculated, in some measure, to perform the office of the part destroyed. But I imagine, when any intervertebral ligament has been considerably eroded, no new generation would answer the purpose of restoring the

functions of the part: an immoveable stiffness would remain. And, I imagine, physicians have been deceived when they have supposed anchyloses of the joints to be cured. They must have been instances of an inspissated synovia, rather than an erosion of the ligaments. The yolk of an egg, beat up with water, has been a frequent prescription in these cases. But can any one believe, that rubbing a knee a few times with this medicine would remove an anchylosis of that joint, proceeding from an erosion of the ligament? The friction used in making the application may have some effect in exciting the activity of the vessels, in slight stiffnesses of the joints. But that it would restore the ligament eroded, is too incredible for me to believe.

The scrophula has been treated by writers, as a complaint that always primarily affects the conglobate or lymphatic glands. On the first appearance of this tumor, I was not a little surprised, and more especially when I opened it, and found it to be of a scrophulous nature. The glands of the mesentery had undoubtedly been tumified previous to the appearance of this swelling. But no external appearance of a tumor occurring in the beginning of his complaint, gave occasion to my mistake in supposing it a rheumatic affection. And, indeed, the connection between an incurvation of the spine and the scrophula, exceeds our present pathological knowledge. From an incident that lately fell under my observation, I am led to think, that a scrophulous affection may happen without primarily affecting the glands. A boy about fourteen years of age, in good health, of a temperament indicating a scrophulous habit, was attacked with a large tumor in the fleshy part of his thigh, which continued painful and inflamed until it suppurated, broke, and discharged. The discharge resembled exactly the discharge of the patient whose situation and complaints I have thus far related: but in a short time it healed up and left the boy in as good health as it found him. In this case, no emaciation or symptom appeared, from which we could infer an obstruction or affection of the mesenteric glands. And I am led to believe, that many cases occur which cannot be referred to any primarily glandular affection.

Since I first discovered this case to be scrophulous, I have never doubted that the foundation of it was laid in his original stamina. Neither has the novel, unfounded sentiment, respecting the transmission of diseases, from parent to child, gained my assent. The circumstances pertaining to this complaint, all serve to establish the ancient opinion. And I am firmly persuaded, that an accurate and vigilant attention to circumstances will more surely lead us in the steps of truth, than consequences drawn from theoretical or analogical reasonings.

The father of this child was a most healthy man, and the mother too, previous to her pregnancy with this and the child formerly mentioned. She had three children before this, as healthy and robust as any I ever saw. My mother, previous to her marriage, was sometime affected with a cough, and her physicians and others thought she actually had a phthisis. She so far, however, recovered as to enter into the holy estate of matrimony, and bore five children; two of which died with the scrophula, one with the phthisis pulmonalis, and myself and the other surviving have been greatly afflicted with rheumatic complaints. Numerous instances have fell within my observation too notorious and circumstantial to be refuted by the reasonings of speculative theorists.

Before I close this narrative, it will be proper to mention, that this child has not grown any since the first attack of his complaint; that he has, at times, been afflicted with rheumatic pains in his limbs, and lately with the most excruciating pains in his eyes. How this disease will terminate is beyond the science of prognostics to predict, and almost beyond conjecture to imagine. That he will arrive to the age of puberty, (at which time it has been supposed the increased vigour of the solids often removes the complaint) appears to me improbable. For it does not seem likely, that the vessels, when their growth has been so long interrupted, will acquire such an additional degree of vigour as to remove the disorder and bring him to the size of manhood. And if his fibres should acquire vigour sufficient to eradicate the disease without increasing the growth, the spine will remain immoveably incurvated, and he will be a standing spectacle of human misery, and a reproachful example of medical impotency.

ARTICLE V.

As the following Case of the successful Application of Caustic to a Stricture in the Urethra tends, as far as a single Fact will go, towards settling the contested Question between two celebrated Writers (Hunter and Bell) in regard to the Propriety of that Practice, it may, perhaps, have some claim to a Place in your Repository; *if you think it will pay for its Page, it is at your Service.*

From your Friend,

VALENTINE SEAMAN.

To the Publishers of the Medical Repository.

JOHN WALL, a middle aged man, was admitted into the New-York Hospital 31st of 10th Month, (October) 1796, affected with a *fistula in perinæo*, and an almost intire *obstruction* of the natural passage of his *urine*, so that he could force out but a few drops by the urethra, the main body of the water flowing constantly and involuntarily through the opening in the perinæum.

The complaint having originated from a syphilitic affection, which he had contracted about twelve months before; and, notwithstanding he had been, as he said, under the care of different physicians since that time, whereby he most probably had every venereal taint eliminated; still, to be clear on that head, he was put under the usual mercurial remedies.

I made several attempts, and finally succeeded *once* in passing a small bougie beyond the stricture, but which I was not able to repeat. All sizes, and all the methods generally recommended, were tried, in the course of several days, to gain a second entrance beyond the obstruction, but all in vain. In this situation a choice of but two means remained, either to lay open the urethra by an incision at the place of the obstruction, so as to permit a bougie to pass it, or to endeavour to diminish the stricture by the application of caustic, as recommended by Hunter (Treat. on the Ven. Dis. part iii. chap. 2.) The former method "*being attended with so* "*much pain, and such uncertain success,*" as acknowledged by its most partial supporter (Bell), determined me in a preference for the latter: accordingly the guarded tube was carefully passed up the urethra till it met the obstruction, which was just before its curve, the plug then was withdrawn, and the caustic, properly secured in the portcrayon, introduced, and kept applied, for fear of inducing too much irritation, only forty seconds. Immediately upon the withdrawing of which the urethra was washed out by an injection of lukewarm water, previously provided for the purpose.

This operation, which did not produce any great degree of pain or irritation, was repeated every second day, for four or five times, when the patient was enabled to pass his water, in considerable quantities, by its natural outlet, and bougies, could be introduced without any difficulty.

By the constant use of the bougies, which were gradually enlarged, as his complaints would admit of, the discharge of urine by the urethra became more and more free, while that by the fistula diminished, so that in the course of a few weeks it not only intirely stopped, but the fistula itself closed up, without any application whatever to the part: and on the 15th of 12th Month, (December) he was discharged, cured of both his complaints.

ARTICLE VI.

CASE OF MANIA SUCCESSFULLY TREATED BY MERCURY.

By E. H. Smith, *Physician.*

MARY MATHEWS was admitted into the New-York Hospital August 16, 1796. She was a native of Ireland, whence she had lately come, and had been in America but a few weeks. Her person was thin, small, and delicate. She was 17 years old. The accounts given by her brothers, mother, and herself (since the restoration of her reason,) though quite incomplete, agree in representing her as having always been a feeble girl; and, as far as I can comprehend them, subject to something like hysteria—though never with that disease in the form of fit or convulsion. She describes it as a wind rising up from her stomach, a struggling for breath, a sensation like swelling of her tongue, heat in all her body, and an universal uneasiness. No more distinct information can be obtained from her relations. She was in good health, from the time of her arrival, till her present illness.

About a week previous to her being brought into the hospital, as she was walking in a field in the country, some miles from town, she was suddenly taken insane. She was carried home that day, or the next, and her symptoms continued to increase in violence till after her admission.

Her friends, who conducted her here, assured me that she had eaten nothing, nor taken any kind of nourishment for a week. Notwithstanding, her efforts were so violent, that it was necessary to cause her to be confined in a cell, and to be strait-waistcoated. In all this week nothing had passed her bowels; but she voided urine as in health. Repeated and careful attempts were made, and for near an hour in my presence, to administer food, with no effect. It was forced into her mouth, but instantly rejected. The same was the case with the purgative powders which were directed: and no exertion, proper to be made, was equal to the injection of a clyster.

Between the mornings of the 16th and 18th, the patient completely tore off and rent apart three strait-waistcoats; neither of which, probably, could have been rent by the utmost combined efforts of two strong men. In one instance, after freeing her arm, she forced off the grating of her cell, leapt into and ran across the yard, jumped into the washing-room, and drank some dirty suds.

This was the first thing she had been known to swallow since her disease commenced. Five or six persons were, with difficulty, able to force her back to her cell.

As it was impossible to keep her covered, she went naked. Of the indecency of her nakedness she appeared to have no idea, for she took no pains to conceal it. She disposed herself in various and scarcely conceivable attitudes; and continued, for hours, in postures which well persons could not have assumed, much less have rested in. Meanwhile, she noticed no person, not even her mother or brothers; but divided her time in singing methodistic hymns, and putting up short prayers. Yet her insanity did not appear to have any connection with religious ideas, further than the repetition of these verses and prayers,—to which she had probably been accustomed from her infancy, and now repeated like any thing else strongly fixed on the memory. That her disease did not proceed from any insane religious impression is evident from her never having expressed any anxiety before, during, or after her illness. Nor did she seem to suffer from apprehension of any kind. Her only uniform exertion, beside that which has been noticed, was to escape, when the door of the cell was opened. But she shewed no uneasiness to effect this, when it was shut; nor did she, at such times, nor at any time, endeavour to hurt her attendants and visitants. No artifice of theirs could engage her attention.

August 18th. She had now been eight or nine days without food, and without a motion of the bowels; during which time she had taken no sleep nor repose, and the violence of her efforts, in singing, &c. was not sensibly diminished. As the vital energy seemed principally determined to the external muscles, leaving the stomach and intestinal canal, in particular, in a state of apparent torpidity, it occurred to me that, notwithstanding her long inanition and violent exertions, any thing which would considerably diminish the muscular force would tend to equalize the distribution of that energy, and would promote a cure. I was, therefore, desirous of bleeding her. But it was impossible to use the lancet. She bent her arm; and no force could open, and preserve it steadily unbent. We had recourse to the cupping-glasses, which were applied to the occiput, temples, and forehead; and about six ounces of *black blood* taken away. After this she was more calm, for some hours, and willingly took about a jill of broth or gruel, that was offered her. From an expectation that she would continue in her present state, and an apprehension that the sudden use of much food would injure her, the nurse gave her no more at that time, and even neglected to administer the purging powders that had been directed. The patient soon relapsed into all her violence, and opposition to food; and would take only some

cold water, which she craved, and which caused her to vomit. She threw up only the water.

On the 20th, the cupping was ordered to be repeated; but, from various delays, was not performed till the 22d; when a small quantity of blood was taken away, with no other effect than to make the patient faint.

23d. No material alteration. She has taken no food; has not slept; and, though her efforts are as constant as ever, her strength is very much diminished. I resolved to try the effect of salivation; hoping that if I could succeed in exciting a powerful action in the absorbent system, it would divert a part of the vital energy from the muscles, and awaken the torpid power of the brain, stomach and bowels. Three drachms of strong mercurial ointment were accordingly rubbed in by the morning of the

24th. And her gums were now slightly affected. As this soreness of the mouth came on and increased, she grew calm and rational: took food, purging powders of jalap and calomel, and an injection. She also had her cloaths put on, and was removed on to a clean bed, in the nurse's room. The mercurial friction was discontinued.

This interval of reason was only of a few hours. She became as violent as ever; broke every frangible article in the nurse's apartment; and again tore off her cloaths. She was re-conducted to the cell. The same evening her cathartic operated freely, and brought off large masses of dark and hardened excrement.

25th. The mecurial frictions were renewed. They excited a gentle salivation, and brought back her reason.

26th. This day she was removed into the principal ward for women, and proper medicines and regimen were directed for the restoration of her strength, which was very much impaired. For several days she continued to be exercised with occasional turns of anxiety and distress, such as accompanied the recurrence of her original complaint, after the first interval. These were particularly troublesome about two o'clock in the afternoon, when there was increase of heat, and in the frequency of the pulse. But she mended gradually till the 7th of September, when she was well enough to return to her family. Several months after she applied to the hospital for some trifling complaint, for which the physician in attendance did not think proper to admit her; but she had not then suffered any relapse into insanity.

After the recovery of this patient, she said that she was sensible of her nakedness at the time, of the cupping, and some other circumstances. She attributed her conduct to witchcraft, or something of the kind, which made the wind in her stomach, that she used to complain of, ascend into her head; and she said the heat of her body was so intolerable that she could not endure the

least covering. But her extreme ignorance embarrassed her account so much, that it was difficult to determine precisely what she meant, and how much she might be depended on.—Her skin was never remarkably hot during her insanity; but after the return of her reason, her feet were affected with such a burning heat, that they could only be relieved by placing them in cold water; and she was troubled with irregular flushes, over her whole body, for several days.

I FEEL a greater interest in making the foregoing case public, as, since it came under my care, I have learnt that an insane person, who refused food, starved to death. The facts here related may, at least, suggest a probable resource against an event so melancholy to the friends of the sufferer.

With respect to the disease of Mary Mathews, which I have called *mania*, it may perhaps be questionable whether it has been denominated so with propriety.—It deserves to be remembered, that the whole duration of her illness, as near as we can determine, was fourteen or fifteen days. Is it possible that this was its period, that it then ceased, from having completed its course, or that it was more easily inclined to stop at that time than another? Is it possible that this is a variety of hysteria?—of intermitting fever, which sometimes assumes the shape of mania, and to which the quotidian exacerbations, after the insanity was removed, may be supposed to correspond? Or, is it possible that the cause of Yellow Fever, whatever it may be, which, in different persons, affects different parts of the system, and appears under almost every form of disease, to whose operation foreigners seem peculiarly liable, and were so this year, could have excited the mania of this girl, whose period is that of many fevers, and whose cure, supposing it to have been effected by the salivation, is not hostile to the supposition?

The reader must determine which, or whether any of these conjectures deserve consideration.

ARTICLE VII.

Further Facts tending towards an Explanation of the true Operation of ALKALIS *and* LIME *upon other Substances.*

In a Letter from Dr. MITCHILL *to* THOMAS BEDDOES, *M. D. dated New-York, September* 15, 1797.

WHAT I wrote to Dr. Percival on the 17th of January, 1797, was an attempt to reduce the phenomena of alkaline remedies and neutral salts, in febrile distempers, to a general principle, by shewing how they overcame or expelled the putrid miasmata, or the contagion which induced the symptoms. Since that time, when, in the spring of the present year, it was agitated among the citizens of New-York, whether manufactories of soap and candles generated pestilential air, the facts which presented themselves to my view led to a conviction, that calcarious earths, alkaline salts, animal fats, and vegetable oils, attracted the matter which imparted to the atmosphere its epidemic and sickly influence, and consequently neutralized or deadened the septic effluvia which were the cause of fevers. (Case of the manufactures, &c. stated and examined.) And more recently still, on considering these noxious exhalations, in relation to soils, manures, and vegetation, it seemed obvious, that lime and alkalis could repress them; and, by so doing, did purify the air and fertilize the land; and were thus serviceable in agriculture, not by any septic influence they possessed, but, in a considerable degree, by neutralizing the septic (nitric) acid, or acid of putrefaction. (Medical Repository, p. 39.) Indeed, the united force of the facts afforded by the materia medica, by arts and trades, and by agriculture, prevailed over all the former notions I possessed; and, as I believed, did exhibit in evidence too strong to be resisted, that the principle I had laid down was grounded upon a very broad induction of facts.

Besides the considerations alluded to, my opinion of the power of lime to absorb, and, in some degree, to neutralize the fluids produced by putrefying animal substances, was strengthened by reading GRAYDON's paper concerning the fishes inclosed in the limestone of Monte Bolca, near Verona. (5 Trans. of the Royal Irish Academy, p. 281.) Here is a mass, or part of a stratum of calcarious rock, which contains, not the mere impressions or likenesses of animals, as shistic fossils commonly do, nor *petrifactions* of them, as is generally the case with calcarious rocks, BUT THE ACTUAL REMAINS OF DEAD FISHES, of their natural size and

figure, PRESERVED LIKE MUMMIES, and so complete that their genera and species can be perfectly ascertained. The lime, in this case, is penetrated by animal matter, which seems to have imparted to it the quality of emitting a fœtid and unpleasant smell when scraped or struck. No analysis that I know of has been made of the rock: but among the matters with which it is charged are evidently these:—whatever of fixed air has been formed by the union of oxygene with carbone, during the putrefaction of the fishes, has been attracted by the calcarious matter, and consolidated with it; the whole of the water produced by the junction of their oxygene with hydrogene, has been absorbed by the porous earth; all the oil formed by the union of their hydrogene with carbone, has been drunk up in the same way; and the septic acid, formed by the combination of their azote with oxygene, has likewise become embodied with the surrounding lime. Thus, as fast as any thing fluid was formed, it was immediately imbibed; and as the fluids formed by putrefaction are thus combined with the calcarious stone, the residuary matter of the animals is left in a dry and somewhat firm condition, resembling mummies; the lime having had, as it were, an embalming effect, or, at most, having literally acted the part of a sarcophagus. In particular, it may be considered, that the total decay of the bodies of the fishes was prevented by the absorption of the water and septic acid produced during the first stages of corruption.

Yet this preserving power of lime is not peculiar to it, for it belongs to alkalis too. DEAN HAMILTON'S experiments on the power of the *fixed, caustic, alkaline salts*, to preserve the flesh of animals from putrefaction (ibid. p. 319) are sufficient of themselves to correct many of the mistakes we labour under in respect to alkaline salts and putrefying bodies. They shew that caustic pot-ash possesses a power of preserving animal flesh from corruption, fully as remarkable as the antiseptic power of caustic lime. It preserves flesh incorruptible, though it has been generally believed caustic alkalis would consume it. Flesh preserved in this way is so durable, that after twenty-two years keeping it remained unaltered, when broken, the parts hung together by fibres, and looked like a piece of plaister taken from a wall; the fibrous or stringy parts of the flesh not seeming to have been corroded or dissolved by the salt.

The strong *antiseptic* powers, possessed in so high a degree both by quick-lime and caustic alkalis, must induce a change of opinion relative to their effects upon the dead parts of animals. They are antiseptics, and particularly so, for this reason, among others, that they attach and neutralize that great destroyer of organized bodies, the septic acid.

Such a change of opinion would probably have happened long

ago, if the leading medical characters in Great-Britain had attended to a hint of the Italian LANCISI, and improved upon it. This attentive observer of the noxious exhalations of the marshes around Rome, entertained no doubt of the efficacy of *quick-lime* and soda to correct pestilential miasmata, "*non enim dubitamus*," he writes treating of the tanning of leather, *quin lixivium ex viva calce paratum et pulvis myrti et sodæ, quibus pelles abstergantur et condiuntur*, PESTIFERUM MIASMA POSSUNT CORRIGERE (De bovilla peste, par. ii. cap. 2). It is strange that the effects of lime-water, employed in tanning, to preserve the hides from putrefaction, has not been more attended to by philosophical inquirers. And it is no less strange that the power of lime, to destroy putrid vapour (septic acid,) if any should be formed, during the preparation of the skins, and thereby prevent the business from growing unhealthy, has been almost intirely overlooked, though the facts are so plain and palpable. The antiseptic operation of lime keeps the pelts from turning to pestilential air, and instantly attracts to itself every particle of it, if any should be formed. It exercises a like power upon water put into casks for mariners' use at sea. The good effects of lime-water in putrid scurvies, and some other diseases, to which seamen are liable, have been so evident, that ALSTON, in 1752, under a conviction of its beneficial tendency, recommended one pound of fresh, well burned quick-lime, to be put into a hogshead of water, and to be used as common drink by the diseased, and by way of prevention for the healthy; (Dissertation on Quick-lime and Lime-water)—and also to put some of it in the ship's well, to prevent the putrid steams and foul air rising from thence. (Lind on the Scurvy, p. 442).

The disposition of lime to preserve animal substances is further evinced by the incrustations and petrifactions so plentifully to be found in caves and quarries of calcarious earth, and so frequently seen in the collections of the curious. All nature is full of this kind of evidence. There is, therefore, no necessity of mentioning, in detail, the petrified serpents, toads, and almost all sorts of creatures, that have been found embalmed in lime-stone.

But, though alkalis are such great resisters of putrefaction in carcases or their parts, caustic pot-ash produces some effects upon the living human body, which are not generally known. By attending to them, it will appear, this latter may destroy life, though not by inducing any form of malignant or pestilential disorder. Pursuant to the law which provides for the inspection of pot-ash and pearl-ash in the city of New-York, large quantities of those articles are deposited in the store-houses of the persons appointed by the government to examine them. The quantity of these salts is so great that the inspectors are obliged to employ frequently several men to assist them, in the capacity of clerks and labourers. In order to

make a complete inspection of the alkalis, it has been oftentimes judged necessary to empty the barrels in whole or in part. During this operation, it generally happened that some of the materials flew about in the form of dry powder, and passed with the air into the nostrils, mouth, throat and lungs of the bystanders. The effects of the inhaled powder of caustic pot-ash are well worthy to be noted. One of the inspectors ascribed to it the purity and soundness of his teeth, and relief from the sensation formerly caused by an acid in his stomach. His decaying teeth ceased to rot any more since he applied pot-ash to them. But much more serious was its operation upon the steady labourers. Sneezing, coughing, and impeded respiration were among the first of its effects. Spitting of blood sometimes came on afterwards. The lungs, if still exposed to the saline dust, became more and more disordered, the strength diminished, and, as the disease increased, the persons were affected as with a sort of consumption, and died. The death of a number of men that had worked in the pot-ash stores is thus accounted for by the inspectors. It has been common to have a considerable part of the work done by slaves; for free labourers, who are acquainted with the nature of the business, engage in it reluctantly, and many of them altogether refuse to work at it. Apprehensive of the danger which arises from starting all the contents of every barrel upon the floor, the inspectors have been reduced to the necessity of discontinuing this part of the practice in some degree. These caustic alkaline atoms, when they are inhaled and dissolved in the mucus of the passages, are thus productive of a lingering indisposition, in which the lungs particularly suffer; but they have never been known to excite yellow fever, or any set of symptoms that resemble it: so far is this from being the case, that during that epidemic sickness which has so often visited the city of New-York, I could discover no instance of the inspectors and their assistants having hitherto been infected by it.

The information you gave me in your letter from Clifton, of June 15, concerning the cure of diabetes mellitus by hepatized ammonia, or by volatile alkali alone, is very interesting. In addition to the plan I suggest the propriety of prescribing lime in preference to ammoniac. The reason of my hint is this: It is said at Schoharie, in the state of New-York, that in that part of the country, where much sugar and molasses is made from the juice of the maple tree, (acer saccharinum) and used plentifully by the inhabitants, diabetes is a frequent disorder. To get relief from the complaint, it is a common practice to take astringents, and that the drinking plentifully of *lime-water* in addition to them, often wrought a cure. This fact leads to a little speculation. The sugar-making process is familiar to many plants. Diabetes seems a sugar-making operation going on in animals, and to be in them a

formidable disease. The benefit derived from lime leads to a suspicion of the presence of the oxalic acid. The Schoharie people are cured by lime, and this probably acts by neutralizing the acid of sugar, and checking that morbid action of the vessels which favours the saccharine secretion in men. Now, from the known stronger attraction of lime for oxalic acid than ammoniac or other alkalis possess, is there not a good reason for preferring it? I wish you would mention it to Dr. Rollo.

The fact now related is connected with another still more important. The part of the country alluded to is underlaid with limestone and calcarious marle, and is remarkable for its freedom from epidemic, intermittent, malignant, and pestilential fevers. This appears to be the case in Tenesee, Kentucky, and all our Western Country, where lime constitutes extensive strata. These disorders, when they break out, happen sporadically from an improper state of the alimentary canal, bad management about the house, or some such cause. But the universal spread of such distempers among the people is prevented by the lime, which attracts and neutralizes the septic and pestilential vapours, turning with them to calcarious nitre, fertilizing the soil, and rendering the settlements there friendly at once to animal and vegetable life. The lime with which the water is charged, has, when drank, a like operation on the contents of the stomach and bowels.

In order to illustrate the effects of alkaline substances on the acid of septon, I have summed up my ideas in the following verses, which I send along for your amusement.

THE DOCTRINE OF SEPTON.

Attempted after the Manner of Dr. DARWIN.

Putrefaction of animal and vegetable substances.

GNOMES! you beheld, with pity and with pain,
Organic relics strew the fertile plain:—
Whatever Forms, with vital warmth endued,
Lords of the earth, or tenants of the flood;
That sport, or bask them, in etherial day,
Or wind their humble, mine their darksome way;—
Of juicy herb, gay flower, and piercing root,
With scent ambrosial, or hesperian fruit;—
Subdued by Death, whose fury nothing spares,
Turn back to *Earths*, or change again to *Airs*.

Excitability, stimulus, excitement, & exhaustion.

You saw where embryo germs to being start,
The brain almost coeval with the heart:
You viewed, no less a miracle indeed,
The pre-existing plantule in the seed:

Saw their nice organs, ere the natal hour,
Acquire a portion of *sensorial power;*
Alike appear with *irritation* fill'd,
As *stimulus* along their fibres thrill'd;
Excitement springing from this mutual strife,
To sense and action wake unfeeling life;
Till, aged grown, the *exhausted* fabric drops,
Each function fails, and every motion stops.

Oxygen, *the principle of* excitability; septon, *the principle of* diffolution.

Man's constitution thus full well YE knew,
From OXYGEN its life and vigour drew;
Whence tinctur'd all its grosser parts refine,
By fire promethean, energy divine;
YOU saw, with wondering eyes and rare-drawn breath,
Infus'd the peccant principle of death;
Grim SEPTON, arm'd with power to intervene,
And disconnect the animal machine;
In our first Parents' breast, the fiend Dismay,
And cherub Hope, contending for the sway:—
With like affright, astonish'd NATURE saw
Giants earth-born dispute celestial law,
TITANIAN POWERS defy the GODS above,
And fierce BRIAREUS dare the throne of JOVE;—
Thus fair CREATION, almost burst in twain,
When PAN, incens'd, disputed EROS' reign;—
So HERCULES had nearly swerv'd from truth,
When VICE from VIRTUE sought to tear the YOUTH;
Hence MANICHEES, by twilight Reason view'd,
Two self-existent Powers, the *Evil* and the *Good.*—

Effects of septon *and its* compounds, *on the* mind *and* body, *in* producing diseases.

Within the great DISORGANIZER lurks,
And plans, unseen, his undermining works;
Insidious, first, with onset mild assails,
Till *sluggishness* or *unconcern* prevails;
Then, mustering thick his fierce *azotic bands,*
More near, the citadel of life commands;
With phalanx firm, the unconscious prey surrounds,
And deep inflicts his unsuspected wounds:
Beneath their blows the sinking powers decay;
The feet with painful effort trace their way;
The feeble arm irresolutely lies;
A dim suffusion clouds the heavy eyes;
Heroic vigour asks a little rest;
And the high purpose slumbers in the breast.
Next, bolder grown, the *Tyrant,* with a frown,
Bids SCURVY break the blood and vessels down;

LEPRA and SERPIGO attacks begin,
And SORES and BLOTCHES desolate the skin;
Shews greedy CANCER how he best may thrive,
And gorge and feast on human flesh alive;
Tells FEVER, as in ambuscade he lies,
An hundred ways to take us by surprize;
To INTERMITTENT, PLAGUE and HECTIC joins,
And CAUSOS, TYPHUS, SYNOCHA combines;
Possessing each, and all, as war they wage,
Sporadic force or *epidemic* rage:—
—Thus, when of old, as pious men believe,
The SERPENT whisper'd in the ear of EVE,
The subtle fiend a fit occasion sought,
With hellish guile, to poison human thought;
With winning speech seduced her easy faith,
And gave the fruit of knowledge, but of death:—
No more the paths of *Innocence* she trod,
But, *disobedient*, turned away from God;
And felt, sad recompence of duty broke,
Vengeful *Remorse*, thy deep and deadly stroke:—
Hence *Pain*, hence *Sin*, their wasteful course began;
Thro' all her offspring vile corruption ran;
And Man, deprav'd, to *vice* and *error* hurl'd,
Still proves the *Septon* of the *moral world*.

Analogy between septon in the natural, and evil in the moral world.

GNOMES! you descry, with keen, lyncean eyes,
The foul *mephitic vapours* as they rise;
Stand by while captivating SEPTON draws
Unwary OXYGEN to aid his cause;—
—Thus JUNO's charms, entranc'd, the thunderer held,
While her lov'd Grecians claim'd the bloody field;—
—Their *silent* union gives the MONSTER birth,
Who wastes with *septic fury* half the earth;—
—Embrac'd by TITAN, thus, in days of yore,
The fifty-handed giant TERRA bore;
With like destruction, on the ARGIVE TRAINS,
The DELIAN PAIR aveng'd their priestess' chains.—
GNOMES! your quick steps HIS subtle flight pursue,
And hold the many-changing FIEND in view;
YOUR guardian cares his secret arts betray;
YOU ope his dark recesses to the day;
YOU mark his conquests o'er the leafy race;
'Mid haunts of men his treacherous foot-steps trace;
In varying shapes, as best he loves to pass,
Of *heavier acid* or of *lighter gas*;

Production of pestilential fluids, by the union of septon with oxygene.

In all the same, with one base passion fir'd,
In every form, by hate to man inspir'd;
Whose days abridged, in solemn warnings shew,
Life brief in pleasure, still too long in woe.—

Operation of calcarious earths, alkaline salts, oily substances, clay, &c. in attracting pestilential airs.

GNOMES! to impede the DEMON's deadly course,
YOUR BANDS CELESTIAL marshall'd all their force;
From watery caves where shelly nations sleep,
From sinuous bays, from Ocean's briny deep,
YOUR hands collecting spread thro' every clime,
A fair proportion of attempering LIME;
Thro' all the space terrestrial Nature owns,
Of Climates, Colures, Longitudes and Zones,
YOUR search the powerful ALKALIS has found,
And cast, the Earth's circumference around;
The friendly powers of METAL, OIL and CLAY,
With duteous zeal your just commands obey;
With wise dispatch their various stations gain,
And guard the Mine, the Mountain, and the Plain.—

Marsh miasmata, &c. YET UNSUBDUED.

——The conflict o'er, behold on all sides round,
In *neutral* chains, the Powers of Mischief bound;
To lie, unless a rescuing force appears,
In durance strict, ten thousand thousand years;
Except where *swamps* their reinforcements yield,
And light detachments skirmish o'er the field.

Deflagration of salt-petre, the acid decfroyed, and the alkali remaining behind.

YOU saw, ye GNOMES! before this day was won,
Prodigious feats of Chemic valour done;—
YOU saw POTASSA seize a *Septic* foe,
His brawny arms around unclenching throw;
Then, plung'd in flames, as shuddering hosts admire,
Himself unhurt, consume the wretch by fire:—

Use of calcarious nitre for manure.

—YOU saw where CALX a *nitric* miscreant found,
And grip'd, and pull'd, and dragg'd him to the ground;
And there expos'd him, suffering day by day,
To hungry plants a sweet and wholesome prey:—

Combustion of oil by septous acid, & *their mutual decomposition*

—YOU saw, that time, terrific anger boil,
When AQUA-FORTIS met with HEATED OIL;—
Both vanquish'd, falling underneath the shock,
Expir'd in blaze and suffocating smoke.—

I might go on to a greater length with this kind of writing, if I had not already violated the poetical precept, "*Nou famum ex ful-*

gore;" therefore the sample you have now got must suffice at this time. From the gaiety of verse, I return to the gravity of prose.

On a survey of the economy of nature, in respect to alkalis and acids, it appears, that, considering the great number of acidifiable bases, and the combination of these in such a vast number of cases with oxygene, acids would predominate and get the upper hand in creation, unless alkaline substances overpowered them, and prepared them for more ready decomposition by vegetation and other operations. Thus the *carbonic* and *septic* acids which are afforded most plentifully during many putrefactive processes, are not only neutralized by lime, pot-ash, and soda, but in these forms are duly prepared for decomposition by plants, which, on finding the carbonates and septites of either of those substances in the soil, will take as much carbone, septon and oxygene, and of the earthy and saline bases with which they were blended, as the wants of their constitutions respectively require. And in doing this the health and life of animals are protected against the dangerous attacks of that redundant acidity, which would otherwise prevail and destroy the balance of powers in the natural world.

In hope of great additional good to be derived from your exertions to establish pneumatic medicine, I conclude, but not without assuring you, that, &c. &c.

SAMUEL L. MITCHILL.

ARTICLE VIII.

ON THE EFFECTS OF ABSTINENCE AT THE APPROACH OF ACUTE DISEASES.

By EDWARD MILLER, *M. D.*

THE following fact seems to deserve more attention than it commonly obtains.—In a district of the United States, distinguished for the prevalence of the epidemic diseases of summer and autumn, it is often asserted, by sensible and accurate observers, that they are accustomed to obviate the attack of fevers, apparently approaching, by rigid abstinence from food. This abstinence, begun as soon as they perceive the feelings of indisposition, usually known to be the forerunners of fever, is continued till such feelings cease, till appetite is restored, and generally, indeed, till the calls of hunger become importunate. On different occasions, this process is of various duration; sometimes occupying twenty-four, thirty-six, or even forty-eight hours, according to the nature and exigencies of the case. The success of this regimen is commended by such as have experienced it, in stronger terms than it would be proper here to repeat, and, perhaps, stronger than the reality of the case can justify. It is not, however, difficult to perceive that much fallacy may be mingled with this sort of experience. Complaints, similar to these in question, are often transient, when every attention to regimen has been omitted. What criterion then shall be resorted to, for the purpose of distinguishing these fugitive symptoms, originating from indigestion or some still more fleeting cause, from the serious ones, which, if neglected, will usher in a severe disease? This ground of uncertainty is freely admitted. But still it remains probable, that there is much truth in the observation, particularly when we call to mind the number, sagacity, and concurrence of the observers, the accuracy of personal experience, and the multiplied instances which epidemic sickness affords for comparison and discrimination. And I am further inclined to give credit to this observation, because it appears to depend upon principles of the animal economy, which are of great importance, and admit of an easy explanation.

Although the observation above-mentioned comes, in the present instance, from a popular source, the effects of abstinence, in obviating the approach of acute diseases, have not escaped the notice of the most eminent physicians. In the writings of Hippocrates we perceive the strong impression he had received on the subject,

Sydenham is still more explicit. In his account of the continued fever of the years 1673, 1674, and 1675, considered by Dr. Cullen as a variety of synocha, or inflammatory fever, it is asserted that he often cured this, as well as other sorts of fevers, in the beginning, merely by directing diluents, and prohibiting every kind of aliment. Thus he relieved his children and intimate friends (to use his own words) *by making them fast strictly for two or three days.*—And, besides medical authority, we may also adduce, in favour of this mode of preventing diseases, the recommendation and practice of many men of letters, who have adopted it with the greatest zeal. The sedentary lives of such persons, diminishing keenness of appetite, and augmenting the burden of repletion, and their experience of higher intellectual power in a somewhat diminished degree of bodily vigour, may, perhaps, account for their attachment to this remedy.*

The advantages of total abstinence, at the commencement of acute diseases, bear an evident relation to the effects of a temperate and abstemious diet, during the prevalence of fatal epidemics. The long experience of countries subject to the visitation of pestilential diseases, and of several parts of our own country, under the pressure of recent or existing calamities of a similar kind, places the benefits of this diet in a strong light. Not only the caution of individuals, but the habits of nations may be distinguished

* Among many examples of literary persons, who have practised rigid abstinence, and derived great benefit from it, I shall mention the late Rev. Dr. Campbell, Principal of Marischal College and the University of Aberdeen, who died in 1795, aged 71 years. "He had, all his life, a rooted "aversion to medicines. He got the better of every ailment, by a total "and rigorous abstinence from all kind of sustenance whatever; and it was "not till he was attacked by an alarming illness, about two years before his "death, that he was persuaded by his friends to call in medical aid. What "nature could do, she had all along performed well; but her day was over, "and something of art became necessary.—Then, for the first time, he own-"ed the utility of medical men, and declared his recantation of the very "mean opinion he had formerly entertained of them and their art."—Monthly Mag. vol. i. p. 344.

Whatever mistakes may appear in such opinions, it is interesting, in all the concerns of health and diseases, where facts and the unbiassed examination of them are so important, to observe the conclusions formed by discerning men, who are, at the same time, divested of the prejudices incidental to the medical profession. While, on the one hand, physicians possess superior advantages in acquiring knowledge of the nature and cure of diseases, on the other, they are peculiarly exposed to certain sources of error. Systems collect, combine, generalize and interpret facts; but they also, sometimes, distort and mutilate them. Hence the common sense of mankind, and more especially the discernment of vigorous minds, is a necessary corrective of that obliquity with which even simple objects are sometimes beheld by the systematical devotee.

in the comparative exemption from diseases, which they derive from abstemiousness. The French and Spaniards in the West-Indies, and in other warm climates, avoiding the use of ardent liquors, and retaining their usual habits of thin and spare diet, are observed remarkably to escape the dangers incidental to such situations; while the British, carrying with them, wherever they go, not only their plethoric and vigorous habits, but likewise their national predilection for a gross and stimulant plan of living, suffer all the havoc of those baneful countries. From every tropical region similar examples might be brought; and wherever experience has enforced accommodation to the inclemency of a hot climate, we observe people relinquishing all such excesses and grossnesses of diet as can only be safely indulged in the higher latitudes.

The importance of the functions of the stomach in the animal system explains the powerful effects of abstinence and repletion. No animal can exist without a stomach. Deprived of brain, heart and lungs, the cold-blooded animals have been observed to live and move for several hours. The languor of their circulation, their occasional exercise of respiration, and a portion of excitability singularly inherent and inseparable, enable them, while the energies of the stomach continue, to retain life without the aid of those important organs. And, even in the more perfect animals, the functions of the stomach hold so distinguished a rank, that life has remained for some time independently of almost every other part of the body. The range of sympathy which it possesses with other parts, remote as well as contiguous, is so extensive, that it is emphatically stiled the index of the whole system. Besides its importance as the principal organ of assimilation, we observe a great number and variety of effects, salutary, morbid or deleterious, produced by different substances taken into it, and operating on its sensible and delicate texture. In febrile diseases it affords some of the most interesting and satisfactory indications concerning their accession, progress, remission, crisis, and cure.* And, finally, it possesses a mobility, or, in other words, a promptitude to suffer more defect or excess of excitement than any other part of the system.—Such is the organ—such the powerful changes in it, and thence in the whole system, which we propose to excite by occasionally depriving it of the accustomed stimulus of aliment.

Acute diseases invade the body in various ways; sometimes suddenly; oftener by gradual approach: when suddenly, they admit not of prevention by abstinence. It is probable much more may be learned by future observation than is at present known, concerning the distant approach of these diseases, and, consequently, the means of averting the danger they produce. In

* Medical Commentaries, vol. xviii. p. 94.

the meantime, it would be fortunate for mankind if they were disposed to avail themselves of all the notices of approaching illness already well understood. During the prevalence of epidemic diseases, these symptoms should be watched with especial attention; as, upon the proper management of the interval between them and the actual formation of the disease, the prevention of the evil, and the safety of the patient will often depend.

It would be no easy task, nor is it necessary to point out all the symptoms which notify the approach of acute diseases. There seems to be so much reason to ascribe indicial functions to the stomach, that we should generally look to that viscus for the earliest notices of impending mischief. Accordingly some accurate observers mention a peculiar, disagreeable affection of the stomach, difficult distinctly to describe, compounded of nausea and anxiety, as the first morbid sensation. Then follow heaviness, lassitude, languor, debility, oppression, restlessness, head-ach, or giddiness, pain in the back or limbs, perversion of taste, flatulency, irregularity of the intestinal discharge, loss of appetite, or sometimes great keenness of it, low spirits or unusual vivacity, wakefulness or unusually sound sleep.* When such symptoms as these occur, during the prevalence of an epidemic disorder, or after exposure to any of the known causes of acute diseases, it will be advisable to abstain from all aliment for a proper length of time, and if this step should be found unavailing, to adopt such farther measures as the nature of the case may require.

Sensations which precede the invasion of acute diseases, it is well known, are often mistaken for symptoms of indigestion, and treated accordingly. The mischief that must ensue from the use of ardent spirits, the popular remedy of indigestion, at the approach of a malignant fever, or any inflammatory disease, will be readily seen.

All alimentary matter, especially of the animal kind, taken at the commencement of an acute disease, is fraught with mischievous consequences. The powers of digestion are either impaired or totally suspended. No assimilation nor nourishment can take place. The stimulus of the food, immediately on its arrival at the stomach, will be added to the morbid stimuli, previously operating with pernicious violence. Tottering under its present load, the system is forced to sustain new burdens. But this is not the only bad effect of receiving food under such circumstances. The alimentary matter, unsubdued and unassimilated by the powers of digestion, placed in a situation where it must undergo a rapid and noxious decomposition, will form a mass of corruption and acri-

* The ſtimulus of contagion or miaſma, in certain degrees of force, may evidently produce exhilarating and ſoporific effects.

mony, generating and diffusing its poison throughout the whole tract of the stomach and bowels. Can we wonder, after this, to hear complaints of flatulency, oppression and anxiety about the præcordia, pains in the bowels, diarrhœa, &c. in the course of the disease?

Having thus stated some of the ill consequences of food received into the stomach at the commencement of acute diseases, it becomes necessary to avoid these consequences by abstinence or evacuations. Much has been said, by practical writers, on the efficacy of emetics, exhibited at the approach of malignant fevers; and there is reason to assent, in general, to the truth of these assertions. But, we believe, that abstinence may often be advantageously substituted for emetics or other evacuants in such cases, and as their effects correspond in several particulars, and seem to throw light on each other, we shall venture to consider them in a comparative point of view.

Emetics produce the following effects—they empty the stomach and the upper intestines—they accumulate excitability in these organs*—they increase the action of the cellular, pulmonary and cutaneous absorbents—and they promote perspiration.†

It will not, I conceive, be difficult to demonstrate that abstinence produces effects nearly similar. That emptiness is one of its consequences, must be too obvious to require proof or illustration—and that excitability is accumulated by withdrawing, for a time, from any part of the living body its accustomed stimulus, is likewise a fact too plain and simple to be denied. When food and drink are withheld, no person will doubt that the action of the lacteal and lymphatic absorbents of the stomach and bowels is proportionably diminished; and it appears to be a law of the animal

* That emetics accumulate excitability, is proved by their often stopping spontaneous vomiting, by their strengthening digestion, and by increasing the action of the cellular, cutaneous, and pulmonary absorbents, during their operation. Zoonomia, vol. ii. p. 57.

† There are, indeed, other considerable effects of emetics, such as agitation and compression of the abdominal and thoracic viscera, thereby increasing the force of circulation in them, and promoting their several secretions. But these effects appear to hold but little importance in preventing the attack of fevers.—Dr. Darwin, "in his theory of fever, supposes that emetics, "early administered, sometimes cut short the disease, by causing a retrograde "motion of the lacteals, and a consequent diminution of the matter of con-"tagion. Few explanations in his work are of so gross and mechanical a "cast. We conjecture that the blow must be given to the stomach before "the subtile matter is absorbed by the lacteals: we should not be surprized "if these vessels were rendered incapable of action: and does it not appear "more consonant to other parts of the author's reasoning, to suppose that "vomits, in these instances, counteract the exhausting effect of the poison, "by accumulating the sensorial power of the organ?" See Analytical Review for Feb. 1797, p. 139.

economy, that any diminution of the action of these branches of the absorbent system will be compensated by a correspondent increase of energy in the cellular, pulmonary, cutaneous, and other absorbents. Proofs of this fact might easily be multiplied. The effect of *fasting* in exciting a very copious discharge of urine, in dropsy, exemplified in the case of the celebrated Dr. Johnson,* as recorded by Sir John Hawkins, is directly to this point. Dr. Rush, who quotes this case, informs us he has tried the same expedient, in dropsies, both in private practice, and in the Pennsylvania Hospital, and found a confirmation of the fact.† With respect to the increase of perspiration by abstinence, it would, perhaps, be more correct to say, that it prepares the way, or rather creates a disposition in the secreting vessels, on the surface of the body, to be more powerfully acted on by diaphoretic medicines. This class of medicines are all said to exert greater effect, if given early in the morning, about day-break, than at any other time; and this must doubtless be ascribed to the increased excitability of the whole system, at that time, accumulated during sleep. That a similar accumulation of excitability ensues from withholding the customary stimulus of food, cannot be called in question; and it is equally clear that this accumulation is especially produced in the secreting vessels of the skin, owing to the well known sympathy between them and the stomach. It follows then, that any diaphoretic remedy, and even very gentle ones, exhibited in this condition of the system, assisted by moderate warmth, will act with increased energy, and this augmentation of action will be commensurate with the previous augmentation of excitability. Every one must have remarked the heat and glow of the skin, when any stimulant matter is taken, after long fasting; which exactly corresponds with the glow and heat of the skin immediately succeeding the action of vomiting.

But, however useful emetics may be thought at the approach of fevers, they are liable to many exceptions. Some persons, from peculiarities of conformation, or other causes not well understood, take them with great difficulty; in many conditions of the body they are unsafe; and in many cases of incipient fever, their operation might be followed by inconvenient or detrimental consequences.

Abstinence, the substitute here proposed, is fitted for more general application, and possesses the rare advantage of being adapted to obviate the approach, or, at least, to abate the violence of almost all acute diseases. The safety of it can scarcely be questioned in any case. If an inflammatory disease be coming on, few will doubt the propriety of total abstinence for some time, and a restriction to the mildest diet, when nourishment becomes necessary.

* Life of Dr. Johnson by Sir John Hawkins, page 499, 500.
† Med. Inquiries and Observ. vol. ii. p. 180.

To fevers, which are the offspring of miasma or contagion, this treatment is equally applicable. These poisons, whatever degrees of sameness or variety may be attached to them by different opinions, are generally supposed to affect the system by a stimulant operation, and, in arranging the means of prevention, may, at all events, be confidently associated. There seems much ground to suppose, that they ordinarily obtain introduction by the mouth, and, conveyed by the saliva, soon find a lodgement in the stomach.* By abstinence that organ is enabled to maintain a more vigorous combat, to rally all its forces; and, finally, by dint of habit, to disarm the noxious intruder. By indulging repletion at such a moment, by heaping alimentary upon morbid stimulus, the energies of the stomach must be in hazard of being overwhelmed, of sinking into indirect debility, and thereby giving deep root to a violent disease. Abstinence is also one of the most *convenient* means of preventing diseases. No confinement is necessary, no interference with the ordinary occupations of life. If the apprehensions which gave rise to it prove groundless, no trouble or injury is sustained; but the system, set free from an accustomed stimulus, feels a lucid interval, not often experienced by the votaries of luxury, and afterwards returns to the charge with redoubled gratification. If the character about to be assumed by the disease was of a moderate kind, the abstinence alone we suppose to be sufficient to strangle it in the birth; if more malignant, and our easy precaution should prove insufficient, some advantage, and not a trifling one, will at least have been gained. The stomach will, certainly, be in a better condition for the reception of other remedies.

There is scarcely any disease in which the employment of abstinence for some time, and afterwards of a mild, cooling, and spare diet, is so signally beneficial as in catarrh. If the aid of this simple treatment were not so much neglected, we should not so often see catarrh precipitated into peripneumony, or protracted into phthisis.†

To relieve the debilitated state, consequent upon intoxication with vinous or spirituous liquors, nothing is better adapted than withholding, for some time, all aliment. This remedy is the more necessary, as acute diseases are often introduced on occasions of intemperance, and a malignant and fatal character apparently imparted to them from this cause. In cases of this sort particularly, and probably in most others, where abstinence is recommended, the good effects of it will be increased by frequent draughts of cold water, and even of iced water, if cautiously used. But the

* Gardiner's Observations on the Animal Economy, p. 196.
† Medical Observations and Inquiries, vol. iv. p. 289.

cases of topical inflammation should be excepted in this observation.

Chronical vomitings of great obstinacy sometimes occur, in which it is probable a total prohibition of food and drinks, for some time, would afford relief, if any adequate mode of nourishing the body, otherwise than by the stomach, could be devised. Injections and baths of nutritive fluids, or the transfusion of blood from another animal, would be most likely to answer this purpose.

It is probable that, in some cases of fever, the stomach may be so affected by the virulence of contagion, as to become completely paralytic, and unfit for the reception of food or medicines. In such case, every thing received by that organ, if not pernicious, must be intirely useless; and, perhaps, the best means of restoring its power would consist in leaving it, for some time, in a state of perfect emptiness and quiet, and meanwhile conveying nutriment and remedies into the system by other channels.

In diseases of great direct debility, abstinence may frequently prove an excellent remedy. The reduction of excitement in the stomach, far below the natural standard, may often be necessary, in order to prepare for the invigorating operation of succeeding stimuli, which the extensive sympathies of that organ are so well calculated to propagate over the whole system.

Two cautions will obviously occur in the employment of this regimen—first, that constitutions of uncommon feebleness and delicacy, or such as are broken by intemperance, or the decline of life, can safely sustain it only in a moderate degree—and secondly, that it be not allowed, in the case of violent diseases, to usurp the place, and lead to the neglect or postponement of more active remedies. The approach of fevers may be accompanied with such signs of malignity, or the nature of the prevailing epidemic may suggest such well-founded distrust of any apparent mildness of invasion, as to render abstinence alone too weak, too dilatory, and too uncertain for a moment of such urgency. Still, however, it may be maintained, that all other remedies will derive additional force and efficacy from the co-operation of this.

Amongst all the effects of emetics and abstinence, at the approach of malignant fevers, none deserves more attention than the increased quantity of excitability which they collect. The doctrine of the effects of stimulant powers, applied to accumulated excitability, is so luminous and philosophic, rests upon so broad a basis, and is of such important and extensive application, in the conditions both of health and disease, that it can scarcely receive too much consideration. Proofs and examples of this law of animal nature are continually before our eyes. We observe it in the effects of the cold bath—our eyes experience it in passing from a

dark apartment to the light—and still more violently in the effects of too much food or warmth allowed to persons previously subjected to famine or frost. Diseases also furnish us with familiar examples of the same law, in the more powerful effects of Peruvian bark after the previous exhibition of emetics, and the redoubled efficacy of opium in relieving pain, when it has been preceded by venesection and a cathartic.

On this grand principle it is easy to perceive to what extent, and in what various degrees, abstinence may be employed as a preventive and a remedy. In the first place, it will diminish excitement, and so far lessen the amount of stimulant power, upon which the attack of febrile diseases so universally depends; and, secondly, by means of the excitability thus collected, the foundation will be laid for a more effective and vigorous excitement afterwards, when it shall be found consistent with safety. At the approach of an acute disease, the abstraction of a few meals may thus either expel it altogether, or greatly abridge its violence; and, when that is accomplished, the renovated stimulus of aliment, so congenial and salutary, will be sufficient to complete the cure. Common articles of diet are thus rendered active remedies. Powerful medicines, in too large doses, or unseasonably exhibited, are converted into poisons. Just so with respect to the mildest aliment; a certain condition of the system, viz. the fasting condition, changes it into an active stimulus; and another, viz. the starving condition, exalts the ordinary quantities of food to a deadly poison. Arsenic does not more certainly induce fatal inflammation and gangrene in the stomach, than mild food taken by a famishing person in quantity disproportionate to the collected excitability. Stimulants may not only be suited to the state of the system, but the system itself may be adapted, in this manner, to the force of stimulants. Such a remedy therefore as abstinence, possessing so great a range of power, so simple and so accommodating, if it be not allowed to supercede many others, certainly cannot be despised.

If the art of preserving health, and prolonging life, chiefly consist in a frugal and sparing use of stimuli, and adapting them with caution and skill to the fluctuating circumstances of the vital principle, we shall surely find still stronger motives to apply this doctrine at the approach and in the treatment of diseases, when noxious powers of such preternatural violence invade the body, baffle every remedy, and stimulate it to death. The regulation of this vital principle, here denominated excitability, the preservation of it when present, and the restoration of it when deficient, the restraint of excitement within the bounds of moderation, the prohibition of all wasteful and undermining excesses, will, probably, hereafter, at some more enlightened æra of medicine, form a sys-

tem of rules for the management of health and the prevention of diseases, for the enjoyments of sense, and the refinement of intellect, which, instead of the present feverish dream of human life, will present a consummation of improvement and happiness, which we now ascribe to superior beings.

I have thus undertaken to examine a noted popular observation, to inquire into its truth, and to demonstrate its consistency with the most established principles of the animal economy. If I do not mistake, it has been proved that abstinence will be often a complete, generally an useful, and almost always a safe means of obviating the approach of acute diseases. And, in a word, if it were possible to offer to mankind a maxim of universal application to the treatment of incipient fevers, in all their variations and circumstances, I should be inclined to hazard the following aphorism: *When symptoms, denoting the approach of acute diseases, are discovered, abstain, for a proper length of time, from all aliment.*

ARTICLE IX.

REMARKS ON A SINGULAR CHANGE IN THE HUMAN LIVER BY PUTREFACTION.

By Mr. FOURCROY.

[*Translated for the Medical Repository, from the third volume of the Annales de Chimie, p.* 120 *and seq. printed at Paris in* 1789.]

AS yet we are but little acquainted with the intimate nature or composition of the different tissues of fibres of which the bodies of animals are composed. If we except the bones, which have been pretty well analized by Scheele, and the chemists who have followed him, the muscular flesh, the membranes, the tendons, the ligaments, the pulp of the brain and nerves, the parenchymatous structure of the viscera, which have been regarded as almost one and the same substance, more or less elaborated, organized and animalized, are not really, as yet, understood by us; every thing assures us that their component matter is not the same, that the elements which form them are in very different proportions, and that their structure, composed of principles differently combined, is destined for different functions. In attending to what an exact analysis has afforded on this subject, we must be careful to collect all the facts which appertain to it. I have already got together several of these facts from different works. That which I am now going to offer is of a kind capable of throwing light upon the bile and the diseases of the liver.

The late Mr. Poulletier de la Salle, who had devoted his life to the study of the useful sciences, and who cultivated, with particular success, anatomy and medical chemistry, had exposed to the air a bit of human liver, suspended by a piece of pack-thread. This bit, without being destroyed by putrefaction, had, from the beginning, emitted a disagreeable odour; the larvæ of insects, and particularly of the dermestes lardarius, bruchus, &c. had preyed upon it, until at last it became dry by degrees, and turned to a grey and friable substance. It had been thus exposed for more than ten years, and, for the last three or four, it appeared to undergo no new changes. Mr. Poulletier was desirous of knowing its composition, and brought it to my laboratory in May, 1785. At first sight this bit of liver would have been taken for an earthy substance, similar to mineral agaric; but, on a nearer examination, pieces of dried membranes were yet observable, of a brown

colour, as well as vascular strings in an equal state of dryness: rubbed by the finger, it felt fat and soft, like a sort of soap.

The examination we made of this liver gave us results very different from those which its appearance seemed to announce.

1st. We put a small piece of it upon a burning coal; it immediately grew soft, emitting the smell of burning grease; it presently melted altogether, puffed up, turned black, and left a light coaly matter, which, on being strongly heated, was converted into white ashes. This first experiment satisfied us that this animal substance was not reduced to a mere lump of earth, as its first appearance might have led us to believe, and determined us to analize it with greater care.

2dly. Although we could not expect to obtain a very exact result from the distillation of this substance, on account of the small quantity which we had to submit to this trial, we still thought proper to try half an ounce in this way. There passed first some drops of a white water, of a faint smell; a white smoke, more thick, and manifestly oily, very soon succeeded this first product. This vapour condensed into a white concrete matter, sticking to the neck of the retort; it then diffused a very tainted odour; the concrete oil took on a russet colour, and a little carbonated hydrogene gas came over. It appeared that a great part of the liver had passed without decomposition. It was remarkable that the concrete oil, collected in the neck of the retort and in the receiver, had a lamellated and crystallized appearance. The products manifested neither an acid nor an alkaline character.

3dly. One *gros** of the dried liver was put into two ounces of distilled water; a small part only seemed to dissolve in the water by mere heat. This solution was whitish, and opaque; it had somewhat the smell of soap, and afforded a great number of bubbles, by agitation. Its smell was faint, and it turned the syrup of violets sensibly green. Lime-water, without sensibly precipitating it, rendered its odour a little fœtid. The portion of liver not dissolved by the water, was melted by heat, and crystallized on cooling. It gave out a greasy smell, and was inflammable.

4thly. We treated a gros of the dried liver, without heat, with an ounce of the ley of caustic pot-ash; and, by simply rubbing in a mortar, the alkali appeared to act very sensibly upon the substance; there was disengaged a slight ammoniacal smell; and the ley lathered. On heating this mixture, the liquor took on a brown colour, and emitted the smell of heated soap. After boiling about a quarter of an hour, it was filtered hot, it was of a deep russet colour, and passed very well through the paper employed (Papier Joseph.) On cooling, this solution turned to a brown concrete:

* A *gros* is nearly equal to our drachm.

distilled water, of a boiling heat, dissolved it wholly, without leaving any part. The whole substance of the liver had been dissolved by the fixed alkali, even the membranous and fibrous portions we have described. The solution in water lathered strongly on being shook; on cooling, it grew turbid, and let fall some slight curdles of a white colour. Lime-water decomposed it, and precipitated it in abundance of curdles: acids, in like manner, decomposed it, as well as the earthy neutral salts. There was no doubt that the alkali had dissolved a fat oily matter, and formed a homogeneous soap. The indissolubility of the liver in water, or at least its trifling solubility, satisfied us that it consisted, in a great measure, of an oily substance, prone to concrete, very soluble, by means of alkalis, and easily forming soaps with that order of saline substances. We next wanted to know the nature of this oily matter, and to determine whether it was combined with any other animal substance. The following experiment tended much to elucidate this point.

5thly. One gros of dried human liver, reduced by the pestle to a sort of gross powder, was treated with two ounces of alkohol, of thirty-six degrees of the areometer of Mr. Beaumé. The action of this fluid was aided by a moderate heat; after remaining together for two days, the alkohol acquired a russet colour; and a smell, slightly fœtid, was added to that which it had been accustomed to emit. This liquor was filtered, to separate the dissolved portion from the part on which the alkohol had no action. One drop of this sort of tincture, let fall into water, afforded a very thick white cloud, and a very perceptible precipitation. Evaporated from a china saucer, in the heat of a sand-bath, it left a yellowish plate, which appeared, at first sight, to be a resinous matter; water, however, applied to this matter, dissolved a small part of it, gave it a white colour and a clotted appearance, which made it resemble grey concreted oil.

The portion of liver not dissolved by the alkohol, weighed, after being dried, half a gros; the alkohol had, therefore, carried off almost the half of its weight. Four ounces more of this menstruum, applied, at two separate turns, to this piece of liver, dissolved still a part of it: there remained yet near twenty grains undissolved; and we remarked, that this residue consisted of the membranes and vessels which had escaped the action of the alkohol. The parts dissolved by this liquor, in the two last experiments, were, in all respects, like the first: water also carried off a small portion of colouring and soapy matter. The substance separated by water from the solution in alkohol, and precipitated in white curdles, has been separately examined; as it was this upon which it seemed more particularly necessary to fix our attention. The properties it presented have led us to a conclusion intirely different from

what has been hitherto known concerning animal analysis. We had prepared thirty-seven grains of this substance pure, and separated from the part which is soluble in water. It was then of a yellowish colour, soft and greasy to the touch, like a concrete oil. We put it into a matrass, which was immersed in hot water. It grew softer, and intirely melted before the water was boiling hot: when quite liquid, it had a yellowish brown colour, and a faint smell, like melted wax. The fœtor which this substance possessed, before the action of the alkohol, did not exist after its solution in that liquid. After it was completely melted it was run into a china cup, where it formed a solid cake, brittle, and very smooth on the side next the cup. It broke clean, and with a little snap; and exhibited, in its inside, a lamellated structure, and was manifestly crystallized. Hot alkohol dissolved it completely; and it presented all the properties of the concrete animal oil, known, in commerce, by the name of spermaceti, with this difference, that it was not so dry, so white, and so transparent, as the genuine spermaceti, and was easier of solution in alkohol than that animal oil.

6thly. We put two gros of the dried liver, cut into small bits, into a matrass, which was held in water heated to the sixty-eighth degree (185th of Fahrenheit) of Reaumur's thermometer. A part of the liver grew soft, and melted. The portion of liquefied oil, separated by squeezing, and hardened by cooling, presented nearly the same characters as that which had been dissolved in alkohol. The only two circumstances of difference which we remarked were: 1. That the oil pressed immediately from the liver, was higher coloured, and more fœtid, than that which the alkohol had taken up. 2. That it contained a portion of soap, which, in the former experiments, had been separated from the alkohol by the water.

7thly. This portion of saponaceous matter, which we had looked upon merely as an extract in our first experiments, now attracted our attention; it was certain that it was carried off from the dried liver by the water, as well as by the alkohol; but we had as yet no precise idea of its nature. The small quantity of dried liver which we had to make our trials with, was not sufficient for the numerous trials requisite, to come at an exact knowledge of the elements constituting this soap, nor of their proportions. All that we could determine upon this point was, that the soap appeared to be formed from a concrete oil, similar to that procured by pressure, with the conjunction of ammoniac and soda. We even conjectured, that before this liver was completely dried, and before its total conversion into a concrete oily substance, the oil was, from the beginning, in a saponaceous state, intimately combined with soda and ammoniac. What led us to

this conjecture was, that the portion of ammoniacal soap, which remained still in this piece of liver, and whose presence was evident, both from the ammoniacal odour extricated by quicklime, and from its solubility in water, appeared to us to be more abundant in the internal part of the liver we examined, where, by reason of its remoteness from the surface, it had been less exposed to the air.

The facts which have been set forth, as has been already mentioned, were disclosed in the beginning of the year 1785; they were entered in my journal of experiments, and I was waiting for other analogous facts, to connect them with the collected materials of animal analysis. In short, they were looked upon simply as a discovery, having little or no connection with any thing else, until the year 1786, when a great opportunity presented itself of examining animal substances buried in the earth, all lengths of time, from more than forty-two years, down to but a few months, in the cavity of the burying ground of THE INNOCENTS.—During the decay of these bodies, there was formed an oily substance, quite similar to that constituting the basis of the dried liver, whose history has been related.

The nature and properties of these animal substances, changed to an oil resembling spermaceti, united with a large quantity of ammoniac, will make the subject of a memoir, which shall be published in one of the next succeeding numbers of the Chemical Annals. It is sufficient for me to remark, in this place, that the conversion of the substance of the liver, into a concrete oil, analogous to spermaceti, is not the only fact of the kind that has fallen under my observation. A ray of light has suddenly illuminated this part of the analysis of animal substances, and there appears to be demonstrative evidence that, in the progress of a slow decomposition of the dead bodies of animals, many of the soft parts undergo a change similar to that of the liver, which forms the subject of this memoir. I further discovered, that the bodies of quadrupeds, and even those of men, without passing through any real putrid alteration, contained this concrete oily substance, as the brain and vertebral cavity of cetaceous animals: in short, that in certain cases, this oily matter is more abundant, and is amassed in several cavities, where it often forms concretions extremely injurious to the animal economy. I shall explain, in another place, how this fat, concrete substance, which is extracted, in such abundance, from the bodies of animals of the whale kind, differs from grease or adipose matter; I shall endeavour to make it appear, or to determine, how the basis of the soft organs of animals is changed into this sort of oily concrete, and how ammoniac is formed at the same time in the bodies of animals.

I shall here conclude the history of the first class of facts which have presented themselves to me on the discovery of this singular substance, and that the order of the discoveries, to which this, as I may say, has conducted me, might not be interrupted. I had thoughts of adding to this memoir, my own observations on the flaky substance, found by the late Mr. Poulletier, in his human gall-stones, and upon that of some concretions in the gall-bladder, which have not, as yet, been described by physicians.

ARTICLE X.

CASE *of* FEVER, *supposed to have been caused by Exposure to the Exhalations of putrid Beef. Communicated by* SAMUEL OSBORN, *Surgeon to the Federal Garrison on Governor's Island.*

JOHN DEMING, a soldier belonging to this garrison, in the latter part of June last, was excused from duty, on account of a gonorrhœa, which, from long neglect, had become violent and obstinate. A lodging was assigned him in a house without the garrison; the part of which occupied by him, previous to his entering it, had been seldom or not at all visited for some time.

On the first of July he was seized with nausea, rigors, pains in his head, back, and bowels, accompanied by difficult respiration; a thick yellowish fur on his tongue, and quick and weak pulse. His venereal discharge ceased on the first commencement of these symptoms. I gave him, on the first visit, a cathartic of jalap and calomel; after waiting six hours, and no evacuation being effected, an injection was administered, which procured a discharge of a blackish colour and extremely fœtid smell.

July 2d. This morning I found him free from the pain in his bowels, his respiration less difficult, his pulse more full, and not as frequent as yesterday; his eyes remarkably inflamed, and the expression of his countenance wild. He was afflicted with extreme thirst, and all endeavours to assuage it by oranges, lemonade, &c. were ineffectual. At night the pain in his bowels returned, accompanied by extreme heat all over his body. He had stools (by means of injections) similar to those of yesterday. Towards midnight he became more easy.

July 3d. This morning I found him somewhat delirious—his eyes wild and much inflamed, with a slight yellow tinge over his whole body. He was now afflicted with pains in his bowels more violent than before, which were relieved after two copious evacuations. His nausea, which had been incessant to this time, now left him, but was succeeded by a distressing hiccup. At night all his symptoms were abated, and he appeared likely to have some rest.

July 4th. The latter part of the last night, after some quiet sleep, his pains returned with increased violence, with sighing, restlessness, repeated and copious vomitings of a bilious matter, alternating with a hiccup, which was extremely distressing; his wildness and delirium still continued, and the yellowness of his skin was much increased.

July 5th. This morning I found him better, and greatly refreshed by a quiet sleep: at noon he had scarcely any fever.

July 6th. His sleep the last night was interrupted by intervals of restlessness, and nausea. In the morning he was free from fever, but complained of general uneasiness and want of rest, which he obtained in the course of the day, and at night appeared more easy, and likely to rest well.

July 7th. He slept well the last night, and in the morning I found him much better, and intirely free from fever. After this he had no other complaint than general lassitude and debility, the natural consequence of the disease.

From the commencement of my visits to this patient, I was affected, while in the house where he lay, by an extremely offensive smell, for which I was unable to assign any other causes than the attendants' neglecting to remove the fœtid stools of the patient, and other inattentions to cleanliness. The stench continuing after careful dilligence had been used to obviate the supposed causes, I ordered the cellar under the patient's room to be examined, where were found three barrels of beef, in a state of extreme putrefaction. So exceedingly offensive was the smell emitted from this mass of animal putrefaction, that the soldiers who were employed to remove it, were several times forced to desist, for the purpose of breathing fresh air, before they could accomplish their design. On the discovery and removal of this putrid beef, which was made on the fourth of July, and which immediately struck me as the cause of my patient's fever, he gradually mended, and on the thirteenth of July, his venereal discharge, which had ceased at the first attack of fever, without recurring again during the time of his confinement, re-appeared.

It is worthy of remark, that a soldier, attending the patient whose case is above related, was, on the fourth of July (the day when the putrid beef was removed from the cellar) attacked by the usual symptoms of yellow fever, but a powerful cathartic removed all complaint in twenty-four hours, except a weakness, which I attributed to the powerful operation of the medicine.

It may be proper to observe, that the vomitings noticed as having occurred on the fourth of July, were excited by no nauseating medicine, but appeared to form part of the progress of the disease; the patient having been afflicted with nausea, which had increased, with very little intermission, from the first. They (the vomitings) were finally obviated by the occasional administration of Riverius' mixture, but not until after the removal of the putrid beef; the patient's recovery, from that time, was very rapid.

In stating the preceding case, the particular treatment has been purposely omitted, that the facts might appear more connected and precise.

Governor's Island, October 15th, 1797.

REVIEW.

ART. I. *Medical Inquiries and Observations:* Containing an Account of the bilious remitting and intermitting Yellow-Fever, as it appeared in *Philadelphia*, in the year 1794. By *Benjamin Rush*, M. D. &c. &c.

[*Continued from page 79.*]

IN the second part of the work before us Dr. Rush proceeds to deliver his doctrine concerning the proximate cause of fever.

In order to render more intelligible, and to establish his theory of fever, Dr. Rush premises the following general propositions.

1. Fevers of all kinds are preceded by general debility, direct or indirect, as may be inferred from their causes. The causes of fever, which act by inducing direct debility, are cold—the passions of fear, grief and despair—all excessive evacuations, whether by the bowels, blood-vessels, pores, or urinary passages—famine, or the abstraction of the usual quantity of nourishing food.

The causes which predispose to fever, by inducing indirect debility, are heat—intemperance in eating and drinking—fatigue—and certain powers which act by overstretching, wounding, bruising, compressing, burning, &c.

2. Debility, whether direct or indirect, is always succeeded by increased excitability, or a greater aptitude to be acted upon by stimuli.

3. The diminution or abstraction of one stimulus is always followed by the increased action of others.

4. The stimuli, which are the remote or exciting causes of fever, operate on the system in a manner wholly different from the ordinary stimuli of health. In health there is a constant and just proportion between the degrees of excitability, excitement and stimuli. In predisposition to fever, this ratio is destroyed; and hence the preternatural force of stimuli produces irregular action, or a convulsion in the arterial system.

5. The stimuli, which induce the irregular action or convulsion of fever, act, for the most part, primarily upon the sanguiferous, and particularly upon the arterial system.

6. There is but one remote cause of fever, and that is stimulus. Heat, alternating with cold, marsh and human miasmata, contagions and poisons of all kinds, intemperance, passions of the mind, bruises, burns, and the like, all act by a stimulating power only in producing fever.

7. There is but one fever. However different the predisposing, remote, or exciting causes of fever, the unity of it still remains the same.

8. All ordinary fever being seated in the blood-vessels, it follows, of course, that all those local affections we call pleurisy, angina, phrenitis, internal dropsy of the brain, pulmonary consumption, and inflammation of the liver, stomach, bowels and limbs, are symptoms only of an original and primary disease in the sanguiferous system.

Having premised these general propositions, Dr. Rush goes on to remark, that a fever (when not misplaced) consists in a morbid excitement and irregular action in the blood vessels, more especially in the arteries.

This irregular action he asserts to be, in other words, a convulsion in the sanguiferous, but more obviously in the arterial system.

That this is the case, he infers from the strict analogy supposed to exist between symptoms of fever, and convulsions in the nervous system. And in support of this opinion, the analogy is pursued into a number of particulars, minutely compared, exhibiting a coincidence and similarity of phenomena which cannot fail to impress the reader.

From the series of facts and analogies now mentioned, Dr. Rush concludes, that *irregular action, or a convulsion of the blood-vessels*, is the proximate cause of fever. And his whole theory of fever resolves itself into a chain, consisting of four links. 1. Predisposing debility, or weakened excitement of the blood-vessels. 2. An increase of their excitability. 3. Stimulating powers applied to them; and, 4. Irregular action or convulsion.

Having thus delivered his fundamental doctrine, the author proceeds to examine how far it accords with the phenomena of fever, which he divides into two kinds.

1. Such as are transient, and more or less common to all fevers. These he calls *symptoms* of fever, and proceeds to explain the production of them in the following order—lassitude—cold fit—tremors—pains in the head, breast and bones—vomiting and diarrhœa—want of appetite, and costiveness—dry skin, or partial sweats—high coloured and pale urine—suppression of urine—thirst—white, yellow, dry, dark, and black tongue—varieties in degree and distribution of heat—and eruptions.

2. The author proceeds to apply his theory to the description and explanation of what he denominates the different *states* of fever.

Having maintained that there is but one fever, he, of course, rejects its artificial division into genera and species. A disease which so frequently changes its form and place should never have been designated, like plants and animals, by unchangeable characters. Much mischief has been done by nosological arrangements

of diseases. They erect imaginary boundaries between things which are of a homogeneous nature. They degrade the human understanding, by substituting simple perceptions to its more dignified operations of judgment and reasoning.—But, in thus rejecting the nosologies of the schools, the author does not wish to see them banished from the libraries of physicians; when consulted as histories of the effects of diseases only, they may still be useful.

Excess in the force and frequency of the action of the blood-vessels, has been considered as the characteristic mark of what is called inflammatory fever. But there are marks which indicate a much greater excess of stimulus upon the blood-vessels. These are preternatural slowness, intermissions and depression in the pulse, such as occur in certain malignant fevers. In applying his theory of fever to the explanation of all its different states, the author is led, by this view of its inflammatory state, to consider them in the order of their inflammatory character, or according to the force of stimulus which acts upon the blood-vessels. The following appears to be the usual order of inflammatory diathesis, presented to us by nature, in the different fevers described by medical writers.

1. The plague. 2. The yellow fever. 3. The natural small-pox. 4. The malignant sore throat. 5. The fever from the alternate action of cold and heat on the body, appearing with the symptoms of pleurisy, rheumatism, tonic gout, internal dropsy of the brain, and pulmonary consumption. 6. The measles. 7. Catarrh from cold, and influenza from contagion. 8. The common remitting fever, appearing occasionally with the symptoms of colic, dysentery, inflammation of the liver, and internal dropsy of the brain. 9. The scarlatina, puerperal and hectic fevers. 10. The jail fever. 11. The common mild intermittent.

This scale of the degrees of morbid action in fevers is taken from their usual symptoms. But these vary with climate, season, and habit.

In pursuance of the author's plan of substituting in the place of the usual names of fevers, certain definite states which may be applied, with varying circumstances, to them all, he divides them,

1. Into such as affect the whole arterial system, with no, or but little local affection. Under this head are comprehended the *malignant* state of fever, which constitutes the highest grade of inflammatory diathesis. The *gangrenous* state of fever, commonly denominated putrid—the *synocha*, or the common inflammatory state of fever—the *bilious* state of fever—the *typhus* state of fever—the *typhoid* state of fever, inclining more to typhus than to synocha—the *synochoid* state of fever, approaching nearer to synocha—the *synochula* state of fever—the *hectic* state of fever—the *intermit-*

ting and *remitting* states of fever—the *sweating*, *fainting*, *burning*, *cold* and *chilly* states of fever.

2. The author proceeds next to enumerate those states of fever in which there are local affections, combined more or less with general fever. The following belong to this second head. The *intestinal* state of fever, including cholera, diarrhœa, dysentery, and colic—the *pulmonary* state of fever, including pneumony, true and bastard, also catarrh from cold and contagion, and chronic pneumony, or pulmonary consumption—the *anginose* state of fever—the *rheumatic* state of fever— the *arthritic* or *gouty* state of fever—the *maniacal* state of fever—the *apoplectic*, *phrenitic*, *paralytic*, *lethargic*, *vertiginous*, *hydrocephalic*, *nephritic*, *hydropic*, *eruptive*, *hæmorrhagic*, *amenorrhagic*, *hæmorrhoidal*, and *opthalmic* states of fever.

3. In the third and last place, the author mentions the *misplaced* states of fever. The periodical pains in the head, eyes, ears, jaws, hips, and back, which occur in the sickly autumnal months, and which impart no fulness, force or frequency to the pulse, are all *misplaced* fevers. Under this last head are included, the *hepatic* state of fever—the *convulsive* or spasmodic state of fever—the *hysterical* and *hypochondriacal* states of fever—and the *cutaneous* state of fever.

In order to appreciate the importance and difficulty of an inquiry into the proximate cause of fever, it will be sufficient to recollect the learning, acuteness and diligence, which have been often exerted, and the little success which long attended this point of medical research.

Without going back to distant times, it may be observed, that the fruitless labours of Boerhaave, Hoffman and Cullen are in every body's recollection. The disquisitions, however, of these physicians, though they may be pronounced unsuccessful as to the main object of inquiry, have not been intirely without advantage. If their discoveries have not illuminated the paths to be trodden by their successors, at least their mistakes and errors have so palpably blocked up the way, as to make it necessary to shun the course they attempted to pursue, and to explore a new route in quest of truth.

Among such as have more successfully investigated this subject, we are happy to find the author of the present work. His doctrine, in general, is simple, clear, and well combined; he has lopped off many of the mistaken facts of former writers, and cleansed, of much fanciful rubbish, the augean stable of theory; and he has availed himself, in a judicious manner, of some important laws of the animal economy, which have been lately brought to light.

We are not sure that, in every point, we clearly comprehend the meaning of the author. His proposition, that " indirect de-

"bility, particularly if suddenly induced, is always succeeded by "increased excitability, or a greater aptitude to be acted upon "by stimuli," appears to be too general and unqualified. Some of our readers may, perhaps not without reason, doubt the consistency of this with the other leading positions. If it be admitted, that stimuli, in producing excitement, constantly expend the principle of life, or the excitability (and we see not how this can be questioned) it appears difficult to perceive in what manner indirect debility, the effect of excessive stimuli, however suddenly induced, can be attended with an accumulation of this same vital principle.

The essence of indirect debility seems to consist in a deficiency of excitability. The suddenness with which this deficiency may be produced, by the quick and violent operation of stimuli, can scarcely, we apprehend, so far vary the effect, as to give countenance to the supposition, that the excitability is accumulated.

It will be readily admitted, that stimuli may often have a twofold effect, or, in other words, that expenditure and accumulation of excitability may frequently be the simultaneous effect of the same cause; as when certain quantities of alkohol, wine or opium are taken, which, at the same time, exhaust excitability by increasing action, and produce additional supplies of it, by their operation on the sensorium.

From the ever-varying quantities of stimulus and excitability, considering them either in a distinct or relative view, a correspondent vicissitude in the degree of excitement must constantly be taking place. The phrases, *direct and indirect debility*, seem scarcely to express the conditions of the animal system, which they are intended to represent, with sufficient accuracy and precision. They are so much calculated to perplex and mislead, that we suspect much of the obscurity of the passage under consideration may be ascribed to the author's adopting this form of expression. And while these phrases, from their circuitousness and ambiguity, stand in need of constant qualifications to guard them from being misunderstood, we cannot but think it expedient to dismiss them from medical language, and to substitute modes of expression more conformable to the simplicity and truth of the natural fact.

[*To be continued.*]

ART. II. *Medicaments, et Precis de la Methode de Mr. Masdevall, &c. &c.—That is, Prescriptions, and a Sketch of the Method of Mr. Masdevall, Physician of Charles the Fourth, King of Spain, for curing all epidemic, putrid and malignant Distempers, Fevers of different Kinds, &c. &c. with the Means of Prevention. Divided into Paragraphs, for the Use of Families who are unable to procure Physicians.* New-Orleans. *Duclot.* 1796. pp. 48. 8vo.

THIS pamphlet, which is dedicated to the Baron de Carondelet, the Governor of New-Orleans, whose name appears in the correspondence with the commissioner of the United States, now employed in running the disputed territorial line between Georgia and the Floridas, is intended as a sort of popular work, or book of direction, for the use of his Catholic Majesty's subjects in Louisiana. The history of the directions it contains is this: in the year 1783, while Catalonia, in Spain, was laid waste by an epidemic disease, Dr. Masdevall was appointed by Charles IV. to inquire into the cause of the disorder, and its method of treatment, and report thereupon. It appears that this *inspector of the epidemics* (this was his title) was not only very successful in his mission, but published, in 1786, a circumstantial account of what he had done. In this he exhibits the mode of practice which, after twenty years experience, he found to answer best, and which he thinks preferable to any that has been adopted from the time of Hippocrates to the present day.

Mr. Masdevall assures his readers, that so far from having obstructions of the bowels brought on by following his method, they may, with confidence, expect their uneasy symptoms to cease on or before the fourteenth day, if the remedies are employed early. He declares his method a true specific against all fevers of the putrid, malignant, epidemic, quotidian, intermittent, bilious, purple, miliary, ardent, remittent, and pestilential kinds, particularly those proceeding from a putrid air, from the infectious vapours of burying-grounds, jails and hospitals; from ships, and low and marshy places.

An abstract from such a work as this, was thought a proper publication in the king of Spain's American dominions; and, by the approbation of government, grounded upon the testimonials of Dr. M.'s success, submitted to the captain-general of Catalonia, and to the Count of Florida Blanca, the pamphlet now before us has been extracted from the larger work, to instruct the people of Louisiana, in times of sickness, how to employ the means found to be so remarkably beneficial, in one of the principal provinces of Spain.

The publication now before us, being merely a manual of prac-

tice, contains not any description of the country, climate, or disease, but simply states Dr. M.'s method of treating putrid, bilious, or pestilential fevers. His practice is simple, and his remedies few, in these formidable diseases. These we shall particularly lay before our readers. The whole of the internal means Dr. M. prescribes may be comprized under three heads: 1. His ANTIMONIAL MIXTURE; 2. His FEBRIFUGE OPIATE, or, more properly speaking, ELECTUARY; and, 3. His ANTIFEBRILE CLYSTERS. The use, application, and directions, relative to these, make the sum of Dr. M.'s discoveries.

The antimonial mixture is ordered to be made according to the following receipt: take of viper-water five ounces, emetic-wine one ounce, powdered cream of tartar one dram; mix for use. If the viper-water cannot be got, the water of carduus benedictus, borrage-water, or poppy-water, may be substituted for it. As the valuable and operative part of the remedy is the antimonial wine, it may be employed, indeed, by itself; but as the cream of tartar can be procured every where, there will be no difficulty in adding that ingredient to the mixture. The formula now given, is adapted to the ordinary constitutions of the young and robust. The dose is, on the first day, a tea-spoonful, poured out after shaking up the phial, mixed in half a cupful of common water, of the existing temperature; and a cup of the same water is directed to be drunk immediately afterwards. An hour and a half after the first dose, a bowl of light broth, perfectly free from fat, is to be drank, or, if this cannot be had, some thin panado, or gruel made of Indian meal. An hour and a half after this, another dose is to be taken, after the manner of the first, and in that way, alternately, a dose of the mixture and a draught of the soup, for four or five days. And if the patient cannot sleep, the treatment must be continued during the night, as long as he continues awake.

The usual operation of the first or second spoonful of the mixture, is to promote gentle vomitings or stools, or sometimes a slight looseness, with comfortable unloading of the alimentary canal. The succeeding doses will not purge much more, but will promote perspiration very freely, and increase the quantity of urine.

It is hardly necessary to observe that Dr. M. is very particular in proportioning the quantity of the antimonial to the age and strength of his patient. By the help of this mixture, he says, if it be administered *from the beginning of the disease*, the fever commonly abates, so as gradually to go off intirely between the fifth and the eleventh day.

The febrifuge electuary is composed of a dram of the salt of wormwood (carbonate of pot-ash,) a dram of sal ammoniac (muriate of ammoniac,) eighteen grains of tartar emetic (antimoniated tartrite of pot-ash,) and an ounce of finely powdered Peruvian

bark, made into an electuary, with a sufficient quantity of the syrup of wormwood, and is to be employed, after the FIFTH day of the disease, without delay, if the symptoms have not already abated by the use of the preceding mixture. A sixth part is to be taken at a dose, mixed with pure water, and in conjunction with the usual proportion of the antimonial mixture; the patient drinking cool water afterwards and between whiles. If the weather is very hot, the drink should be cold, even as cold as ice. He must then go on taking between the doses of the medicine just mentioned, some of his soup; and thus continue for several days, until the disease relents. If the symptoms do not yield, the doses are to be increased in quantity and frequency.

The anti-febrile clyster is made of the febrifuge electuary ordered in the preceding formula, the whole; of the blessed laxative (we are not certain what this laxativum benedictum is) and of emetic wine each two ounces; mix for ingredients of two injections. Warm water, honey, and oil added to these, are to be administered, and the whole retained in the body as long as possible: and these are to be repeated, according to the exigency of the case, in greater quantity and greater frequency.

Dr. Masdevall rests the cure pretty much upon these remedies, and rejects the cordials, blisters, caustics, and antidotes of the vulgar medical practice. He also is opposed to frequent blood-letting, advising it to be performed upon patients of a sanguineous and vigorous constitution only, and on such no more than once or twice, and that in the beginning of the disorder. What is very curious is, that he considers health and life so intirely residing in the blood, that he affirms, upon the authority of Leviticus xvii. 14. there must be much danger in drawing off that fluid, upon which the existence of life so materially depends.

In the course of the sickness he allows his patients as much cool water or lemonade as they choose to drink; together with cherries, water-melons, strawberries, and any other ripe fruits of the season; and he permits pure chocolate, almond milk, gruel, rice, thin soups, greens, and a little good wine with the fruits. And after the febrile commotion has, by these means, subsided, the alimentary canal should be cleansed by moderate cathartics of neutral salts.

Such is the summary of the Spanish method of cure contained in the publication of this celebrated physician: and we have thought it worth the while to be the more particular in detailing the practice, because, although the inhabitants of the Spanish dominions, both in Europe and America, are subjected to febrile distempers in a great variety of alarming forms, yet the publications of their physicians have rarely reached us, and we are therefore almost wholly uninformed of their method of treatment; we had

rather thought the practitioners of that nation had not published much on the subject, since, in the malignant fever of Barcelona, a few years ago, the Court of Madrid, as is related, wrote the prescription; and, by the command of his Most Catholic Majesty, the physicians were ordered to adhere to it, and forbidden to prescribe any thing else! Amidst the conflict of opinion on the preferable mode of managing these complaints, the reader has it now in his power to judge of the probable efficacy of Dr. Masdevall's method, and compare it with that recommended by other practisers. From some paragraphs in the work before us, we find the physicians of Havannah blamed for not having adopted it, and great benefit derived from the employment of the means recommended in the dreadful sickness of Carthagena, described by Dr. Rodon-Bell in 1787.

As to the prevention of fevers in epidemic constitutions of the air, the author has said very little of any thing remarkable; though besides the common observations about cleanliness, ventilation, &c. avoiding fogs, dews, and night air, he recommends to the well and uninfected, *a vegetable* diet of legumes and ripe fruits, with *very little animal* food. He positively forbids *pork*, but allows a small quantity of good wine. The advocates of temperance and abstinent regimen in pestilential seasons, have here another authority in favour of that system of mild and moderate living which it becomes all prudent persons to adopt in times of such calamity and danger.

ART. III. *Considerations on the Doctrine of Phlogiston and the Decomposition of Water.* By *Joseph Priestley*, L. L. D. F. R. S. &c. &c. Philadelphia. *Dobson.* 1796. 8vo. pp. 39.

THE worthy and indefatigable author of the pamphlet before us, since his arrival in America, continues his chemical labours, and appears as zealous as ever to promote the progress of science. It must give pleasure to every philosophical mind to find the United States becoming the theatre of such interesting discussion as now occupies some of the leading chemists of the day; for this essay, which is addressed to Messrs. Berthollet, De la Place, Monge, Morveau, Fourcroy, and Hassenfratz, the surviving answers of Mr. Kirwan, has been answered, as we shall more particularly observe in its proper place, by the *ci-devant* Minister of the French Republic to the United States, ADET, just before his departure from Philadelphia to France, in May last. We hope that publications upon such subjects, by men of so much ability, will not only prove on which side of the argument the truth lies,

but will be a means of diffusing a taste for this kind of philosophical research among the people of the western world. And we feel a degree of satisfaction in ascribing a considerable part of the increasing taste and prevailing fashion for chemical pursuits, in this country, within a year or two, to the influence and example of Priestley.

Considering that what he had observed in several of his publications had not been duly attended to, or well understood, the author, although he allows he has nothing materially new to advance on the subject, wishes to make one appeal more to the philosophical world: he therefore attempts to exhibit, in one view, the more considerable objections to the antiphlogistic system, with the intention of bringing forward the favourers of the new doctrine to the explanation of these difficulties, by the aid of additional *facts*, and more cogent *arguments* than have hitherto appeared.

Although the Lavoisierian theory had made proselytes of the greater part of the philosophers in Europe and America, and though Dr. Priestley had observed his friends and acquaintance deserting the standard of phlogiston, not merely one by one, but frequently going over to the other side in whole troops, he has never yet found himself disposed to change sides and engage in this revolutionary scheme. Firm in his original persuasion, that the doctrine of STAHL is preferable upon the whole, he adheres to it upon principle; professing, at the same time, with perfect candour, his willingness and readiness to adopt the sentiments of his opponents whenever they shall convince him he is wrong.

The controversy seems to rest here, that the weight of evidence which has been sufficient to induce Kirwan, Black, Gren, and many others of the phlogistic chemists to adopt the theory of Lavoisier, has never yet appeared to Dr. Priestley considerable enough to influence his judgment, or gain his assent. The considerations which have induced almost every other chemist to favour the new system, have failed to work a similar effect upon the mind of our author; and after the conversion of the greater part of the persons who used to think as he does, he calls upon the advocates of the modern opinions to produce, if they can, evidence cogent enough to convince *him*. Though this adherence to a man's particular notions has been termed prejudice by some, we are inclined to give it a more gentle appellation, as we are satisfied there is a becoming and manly spirit in refusing to bow the knee at the shrine of *authority*, and a disposition particularly favourable to the investigation of the *intire* truth, when they upon whom the burden of the proof lies are compelled to establish every point as they proceed, and to take nothing for granted.

The first class of Dr. Priestley's objections arises from the doubts which he entertains concerning the constitution of metals. The

phlogistic system teaches that metals are compounded of a certain earth, or calx, and phlogiston; and that when phlogiston flies away, the splendour, malleability, and ductility of the metal vanishes with it, and leaves an inert calx behind. According to the antiphlogistic doctrine pure metals are not *compounded*, but, on the contrary, are *simple* substances, and owe their calciform state not to the departure of phlogiston, but to the absorption of oxygene, or the basis of vital air. Our author denies that the latter opinion is universally true. He takes quick-silver as an example. Though the mercurius precipitatus per se may be such a composition, yet it is, he contends, unfair to reason from this to all other calces of mercury, and thence to the calces of all other metals, that their calciform state is always derived from their incorporation with oxygene. In particular, the calx which remains after exposing turbith mineral to a red heat, cannot, he says, be revived by any heat, but may be completely reduced by inflammable air, charcoal, iron-filings, or any substances which contain phlogiston. This phlogiston, he contends, is necessary to constitute the calx a metal, in the present case, and consequently in all cases of mercurial reductions; and if a judgment be formed from the calces of metals, and from other circumstances, he says, there are few, if any of them, that do not contain more or less of phlogiston. He observes, that both precipitate per se and minium, if made with proper attention, may be revived without yielding any air; though this, he believes, is owing to their want of water, which he thinks essential to the constitution of every kind of air.

From the exposure of iron to the heat of a burning lens in confined air, the air is diminished, the metal becomes a calx, and something possessing a strong smell is emitted from it. Dr. Priestley thinks this is evidence that iron not only attracts something, but also emits something; and that this, which, with the aid of water, forms inflammable air, is the phlogiston lost by the iron. He denies that the change wrought in the confined air is to be fairly ascribed to the separation and fixation of its oxygenous portion, but that the phlogisticated air is produced by the union of the phlogiston of the iron with the dephlogisticated air: and what is true of iron in this case is true of the same metal in every other case, and of all other metals, as well as of sulphur and phosphorus.

The author is further of opinion, that during the dissolution of metals in acids, the inflammable air produced does not proceed from the water decompounded, but from the phlogiston emitted by the metal. And in the case of the supposed decomposition of water by hot iron, the inflammable air comes from the decomposition of the iron, and not from hydrogene, the imagined basis of water.

Dr. P. next proceeds to examine the celebrated experiments on the composition and decomposition of water. After all that has been done, and all that has been said on this interesting subject, by the able and experienced chemists, who have stated the results of their trials to the public, he still maintains the whole of their experiments are very liable to exception, and that the doctrine of phlogiston easily accounts for all they have observed. He admits, that in the common experiments of making water pass through a red-hot iron tube, *inflammable air*, and *finery cinder* are formed; but he denies that this finery cinder is a mere calx of iron, formed from the union of the iron with the oxygene of the water, but a combination of iron with water itself; and favours the opinion, that the inflammable air is formed from the phlogiston of the iron, united to a portion of undecomposed water, and not from hydrogene, the pretended radical of water.

He mentions that both finery cinder and massicot, are soluble in muriatic acid, without dephlogisticating it, as they ought to do if they contained oxygene, as minium does; and reasons, that if there is a principle in inflammable air, which, being imbibed by the calx of a metal, converts it into a metallic substance, the same principle must exist in charcoal and other combustible substances: that this is phlogiston, and is the same in them all.

The author objects to the correctness of the grand experiment for forming water from oxygenous and hydrogenous gases, and thinks the experimenters have suffered themselves to be deceived, and have imposed their deceptions upon the public; at least the experiments made by himself appear to him to be less liable to exception, and afford a result intirely different from that of the French philosophers; as evidence of this, he affirms roundly, that when the electrical spark is drawn through a mixture of dephlogisticated and inflammable airs, that a highly phlogisticated nitrous acid is produced, which acid is the stronger as the airs are more pure. Though he allows that dephlogisticated and phlogisticated airs will constitute nitrous acid too, and, consequently, both inflammable and phlogisticated airs contain the same principle, phlogiston.

The difference, in short, between Dr. P. and his opponents, on this point, is a matter of fact, wherein counter-experiments are brought forward by each party. The French academicians say, that hydrogenous and oxygenous airs, incorporated by drawing through them the electrical spark, turn to *water*; while our author declares they combine into *smoking nitrous acid*. The parties being here at issue, we suppose further experiments, and by other hands too, must be made to settle the controversy to our author's satisfaction.

Next follow four other objections to the antiphlogistic theory.

The first is drawn from the production of inflammable air when finery cinder is heated with charcoal; the second is derived from the production of fixed air or carbonic acid, in circumstances where not an atom of charcoal was present, and from the formation of it, during animal respiration, wherein if it be alledged to be formed from *carbone*, then carbone is but another word for *phlogiston*; the third is grounded upon the supposed proof, that azotic air is not a compound of azote and caloric, but of phlogiston and oxygenous gas; and the fourth proceeds from the persuasion Dr. P. possesses, that the new nomenclature is a premature publication. If it is intended as a language of science, the facts ought to be known, and the principles established, before appropriate names and terms are affixed to them. In this we agree with Dr. P. and are of opinion, that some parts of the nomenclature have been hastily, some parts incorrectly made out; and that they, at this moment, stand in need of amendment. But notwithstanding his objections, and those of the learned and scientific Keir, in the preface of his Dictionary of Chemistry, we have no hesitation to say, that we think the table of the new nomenclature a beautiful specimen of the analytic method, and an arrangement happily calculated to systematize and simplify the study of chemistry. It is a great outline chalked, and if any of the strokes are drawn wrong, they are the inadvertencies of a master, who, though he did not give the last and finishing touches to the piece, has left us, however, evidence enough that his design was a noble one, and his conception of it clear and strong.

How far the objections of Dr. P. are capable of being answered, will appear from the succeeding article, which will supercede the necessity of any further remarks on our own part at present.

ART. IV. *Reponse aux Reflexions sur la Doctrine du Phlogistique et sur la Decomposition de l'Eau: that is, Answer to the Reflections on the Doctrine of Phlogiston, and on the Decomposition of Water. By. P. A. Adet.* Philadelphia. *Moreau De St. Mery.* 8vo. 1797.

THIS is the performance we mentioned in the preceding article, as published by the Minister Plenipotentiary of the French Republic, between the time of his suspension from his public functions, and his departure from America; and we cannot but think he has usefully employed the interval of leisure which the jarring politics of the two governments afforded him. It too often happens, that public ministers are mere negociators and politicians, and when they are removed from their places, cease imme-

diately to attract or to deserve public regard. In Mr. Adet we have an exception, who no sooner ceases to represent his country in a diplomatic capacity than he undertakes to rectify what appear to be errors in science, and to establish chemical theory upon the unshaken basis of fact.

The answer, like the original piece, is divided into three chapters, in each of which Mr. A. replies specifically to the objections of his opponent.

The author, in the first chapter, on the constitution of metals, considers that the simplicity of metals, and their conversion to oxyds (or calces) by the absorption of oxygene, are already established upon experiments so decisive as to be wholly beyond the reach of dispute. The oxydation of quick-silver in open air, and its revification, without addition in close vessels, with a disengagement of oxygene, together with an increase of its weight in the first case, and a diminution of it in the second, corresponding to the weight of the oxygene, all point irresistibly to the conclusion, that the whole of the phenomena depended upon the fixation and extrication of oxygene. And this is not only true of mercurius precipitatus per se, but of the yellow oxyd of quick-silver, or turbith mineral. He affirms, contrary to Dr. Priestley's assertion, and on the authority of Monnet, Buquet, Lavoisier, and Fourcroy, that this very calx can be revived without addition, as also may the red oxyd of quick-silver by the nitrous acid, and all the other mercurial calces. And even if the calces required the addition of some substance to reduce them, this would be no proof of the existence of phlogiston, for all that could be said fairly, in that case, would be, that the oxygene in the different oxyds, adhered to the metal with different degrees of force, and in some of them so closely, that, not being capable of separation by heat alone, there is a necessity of adding some substance, for which the oxygene has a stronger attraction than it has for the metal.

Mr. A. denies that there is any proof of the addition of any part of the added substance (whether carbone, inflammable air, &c.) to the metal during its reduction. If any thing was added, it ought to be capable of detection by *weight*, but the balance gives *not* evidence of any such thing; and there are numberless cases where these oxyds are reduced to metals without addition of any thing, and consequently the assertion, that phlogiston enters into the constitution of metals, is wholly unsupported by experiment. Besides, how can the reduction of the nitrate of mercury be explained upon the hypothesis of phlogiston? On exposing this, or its red oxyd, to a sufficient heat, a great quantity of oxygene gas is obtained, and some azotic air. This latter, according to the antiphlogistic doctrine, comes from the nitrous acid, of

which it is one of the constituent parts. But, as Dr. Priestley says azotic air is composed of oxygenous gas and phlogiston, he must be puzzled to explain how the quick-silver, on its revival, does, in this case, not *take in*, but, on the contrary, *throw out* phlogiston.

Mr. A. on the authority of the experiments of Van Mons, contrary to the statement of Dr. Priestley, declares the red oxyd of mercury to afford oxygene air on its reduction; and declares, from his own experience, that minium, though newly made, affords a similar product.

The writer then proceeds to the consideration of the smell which iron is said to emit while it undergoes oxydation, and which is adduced as proof of a something emitted by the metal, which something is termed phlogiston; and this odour, he thinks, is peculiar to iron, or to its combination with oxygene, but observes, that further experiments are wanting on the subject, whether the phlogistic or antiphlogistic interpretation is attempted.

Mr. A.'s refutation of Dr. Priestley's idea, that inflammable air is composed of phlogiston and water (p. 50—52) appears to us very satisfactory; and no less conclusive his denial, that the azotic air, remaining in close vessels after the oxydation of iron, is, as Dr. Priestley affirms, produced by the phlogiston of the metal united to oxygenous air. Nothing is emitted from iron, nor from other metals, during their calcination; nor from sulphur, nor phosphorus, during their combustion, which, by its union with oxygenous, can change it to phlogisticated air.

In reply to the objections made to the extrication of hydrogenous gas, during metallic dissolutions, from the water rather than from the metal, Mr. A. has these remarks; "In fact, a metal, as it combines with acids only, when it is in a state of oxyd, "and as it takes upon itself this form by the union of oxygene "only, it must necessarily, as in the experiments mentioned by "Lavoisier and La Place, absorb oxygene, in order to combine "with an acid. But this oxygene can be furnished but by two "substances, either the acid itself, or the water which it contains. "If the oxygene had been afforded by the acid, this would in "part have been decomposed, and, consequently, would have "saturated less alkali after the experiment than before. But since "it saturates afterwards as much alkali as ever, it is clear the acid "has not been decomposed, and, consequently, the metal could "have been supplied with its oxygene from no other source than "the decompounded water."

As to the constitution of *finery cinder*, Mr. A. simply remarks, that it is a mere oxyd of iron, in which the oxygene is so closely connected with the metal, that the muriatic acid has not force enough to separate it.

In the second chapter, Mr. A. expresses his surprize that Dr.

Priestley should raise objections against the composition and decomposition of water, so often repeated and verified by the antiphlogistic chemists; and observes, that all the difficulties raised by our author have been answered already, over and over again, in the writings of the French philosophers. If their publications were to be got in America, he should have but to turn to the passages, and shew how satisfactorily these matters were explained. Circumstanced as he is, he attempts an elucidation of the subject without books: and, 1. He contends, that in causing water to pass through a red-hot gun-barrel, the iron becomes oxydated by the oxygene of the water; and if this oxyd should be incapable of reduction by caloric and light alone, but require the addition of carbone or hydrogene, this only follows from the greater attraction of the oxygene for the iron, in the first case, and is owing to its departure from the metal, and the formation of carbonic acid gas, or water, in the second. 2. Notwithstanding the difference which exists between the black oxyd of iron, produced by the decomposition of water, and the common red oxyd of the same metal, they are still both of them oxyds, for these reasons, that, like other oxyds, they both dissolve in acids without disengaging any thing, and metallic bodies are incapable of combining with acids unless they are previously united to oxygene. 3. Although there is some difference between this oxyd and the common red oxyd, it does not follow that they are not both oxyds; the difference between the two being only owing to the different circumstances under which they have combined with oxygene.

Mr. A. concludes that the whole of Dr. Priestly's objections to the decomposition of water, are capable of complete explanation without the phlogistic hypothesis.

As to the composition of water, our author considers his opponent's doctrine as little tenable. He maintains, that oxygenous and hydrogenous airs, exploded by the electric spark, form water, and not nitrous acid, as Dr. Priestley asserts. And renders it very probable, that in Dr. Priestley's experiments, whether he got his oxygenous air from manganese, minium or quick-silver, that there was a quantity of azotic air, afforded by the oxyds of those metals, together with the oxygenous air, which deceived him, and caused the different results which he speaks of.

The formation of fixed air is to be accounted for, from the carbone held in solution by the hydrogenous gas employed for the reduction of the *metals*.

There are other objections which Dr. Priestley might have brought forward, which Mr. A. thinks could have been as satisfactorily answered, but for further particulars he refers to the experiments of Berthollet.

The third chapter is allotted to the consideration of the remain-

ing objections to the antiphlogistic theory. If finery cinder is heated with charcoal, inflammable air is produced. Charcoal very commonly contains hydrogene; if so, mere heat may expel it.—Where it is declared, fixed air was produced from substances having not a particle of carbone, this must be founded in a mistake, or a misinterpretation of facts. The assertion that azotic gas is a composition of phlogiston and oxygenous air, is combatted with such force and success (p. 82—96) that we feel a persuasion, as well as Mr. A. that, among all the experiments and objections stated by Dr. Priestley, there are none which overthrow the theory of the antiphlogistians, or which invalidate the inferences drawn from their experiments.

ART. V. *An Inaugural Essay on the Yellow Fever, as it appeared in this City (New-York) in* 1795, *&c.* By Alexander Hosack, jun. A. M. of New-York. New-York. *T. and J. Swords.* 1797. 8vo. pp. 40.

THE importance of obtaining correct ideas of the nature and cure of so formidable a disease as the YELLOW FEVER, from which the United States have lately suffered so severely, and which is likely to continue its ravages in our country, induces us to consider the publication before us with a greater degree of minuteness and attention than we suppose generally due to Inaugural Dissertations on practical subjects. We are the more encouraged to an examination of this Essay, as the author informs us, in his preface, that he has "confined himself to what fell under his own observa-"tion, in his attendance at the New-York Hospital, and upon the "private practice of Dr. Samuel Bard and his brother, Dr. David "Hosack." So that we are to regard it as a summary view of their experience and practice in the fever of 1795.

Mr. Hosack first delivers a succinct and correct account of the general appearance and progress of the disease, not materially varying from other histories of it, and therefore not necessary to be quoted. He concurs with preceding writers in affirming, that "it is certain that those who most indulged in the use of ani-"mal food and spirituous drinks were most succeptible of the dis-"ease"—a fact that can scarcely be too often or too strongly impressed on the public mind. But we do not think him equally correct in representing the state of convalescence as so universally tedious and protracted. We are rather inclined to believe that, except in some very peculiar cases, this depended chiefly on the treatment of the disease. Where suitable means were promptly and freely employed in the beginning, the recovery, so far as our

experience extended, was rapid; where they were long delayed, or partially applied at first, recovery was proportionately slow.

The Predisposing and Exciting Causes are judiciously assigned. Concerning the *first*, however, it may be doubted whether the use of camphor, &c. as preventatives, did not rather favour the operation of the disease, by exciting a constant attention to its progress, and by stimulating the absorbents of the nostrils, and promoting the ready inhalation of the morbid effluvia, in places where they abounded; and whether bleeding, purging, and a scanty diet, were followed by any mischievous effects. The current of popular opinion, unfortunately, ran too strongly the contrary way. Numbers, unquestionably, precipitated themselves, by improper indulgencies, into those sufferings from which they might have escaped by the use of a diet less stimulant than ordinary, and a moderate and occasional use of laxatives.

From the consideration of the exciting cause, Mr. Hosack proceeds to the *cure* of yellow fever. This he treats of under four indications—"1st. To prevent the further operation of the poi-"son producing the disease. 2dly. To procure a solution of the "inflammatory stage of the fever. 3dly. To counteract the pu-"trescent state of the body, as described in the second stage of "the disease. And, *lastly*, to restore the tone of the system when "the preceding indications are accomplished."

Under the *first* head, Mr. H. recommends the removal of the sick into pure air, and a careful attention to cleanliness.

Under the *second* indication, he treats of bleeding, purging, vomiting, sweating, and the cold bath; all of which being topics of great importance, on which there has been much discussion, and continues to be variety of opinion, we may be excused for some prolixity, especially as we do not always coincide with Mr. H. either as to fact or reasoning.

With respect to *bleeding*, Mr. H. remarks—"my observation "has been, that the promiscuous use of the lancet was very inju-"rious and unsuccessful. In the New-York hospital it was fre-"quently employed, but in the majority of cases the disease ter-"minated fatally." In reply to this censure of venæsection, we may observe, in the author's words, that "the abuse of a remedy "is no argument against its use;" nor, we may add, is the fatal event of the case in which it was used any proof of its inutility, or that it was pernicious. To determine on the propriety or impropriety of the administration of a proposed remedy, it is necessary to know not only that it has been used, but to what degree, and under what circumstances. "The promiscuous use of the "lancet"—if by this is meant the use of it without regard to circumstance, must undoubtedly be "injurious and unsuccessful." The same may be safely predicated of all other active remedies,

how safe and certain soever in the hands of a judicious practitioner. The particular application of this remark to the lancet, therefore, conveys no precise information. Nor shall we find the argument against it much strengthened by what is said of its failure in the hospital, when the assertion, which is far too general, comes to be examined. Patients, it is well known, were received into that Charity in all stages of the disease. In some instances they died before any efforts could be made for their effectual relief: in many others, where there was but little reason to expect their recovery. In these circumstances, and when it is recollected that nearly half of all who were admitted for fever fell victims to it; that one of the gentlemen in attendance never bled, or, if ever, very rarely; and that his respectable colleague, the present Clinical Professor, directed venæsection in not more than six or seven cases, and in small quantities in every instance, from an apprehension not then subdued, as he very candidly acknowledged, of the consequences—an apprehension which subsequent experience has well corrected:—When these facts are recollected, the event of so few cases, even if fatal, with so limited a use of the lancet, can hardly justify the strong and decisive language of the Essay under review. It may be added, that the practice of gentlemen opposed to bloodletting, and therfore seldom prescribing it, whose professional walk led them principally among the more opulent part of the community, who were natives of the city, and were surrounded by all the conveniencies of life, and in whom many circumstances concurred to render the fever comparatively mild, was by no means the best opportunity of judging of the efficacy and necessity for this remedy. Among the emigrant poor, who were the principal sufferers, the disease was far more ferocious. In their dark and dismal receptacles, in the midst of squalid poverty and neglectful intemperance, by their deserted or crowded beds, it displayed all its virulence, and accumulated all its horrors. A vigorous and prompt method of cure was indispensable; and the oral as well as printed testimony of many physicians in this city, who had the best opportunities for observation, should long since have removed all doubt as to the propriety of bleeding, in the first stage, at least, of all severe cases of yellow fever. And so far from its being probable that the use of the lancet was injurious in those cases where Mr. H. saw it employed, and where the event was unfortunate, it is much to be suspected that this event was the consequence of a too timid exercise of it, or of the subsequent administration of medicines whose operation tended to counteract its efficacy.

Mr. H. having, as we have seen above, entered a caveat against venæsection, proceeds to detail what he denominates "the more "common and successful practice." We shall not stop to con-

trovert the assertion, that the method of cure he proposes was "the most common and successful," but shall rather examine in what it consists. And this we find to be,

"1st. To procure a free evacuation from the bowels"—and for this purpose the author particularly recommends Glauber's salts, "which," he says, "was the most successful, and acquired "the greatest reputation." It is, no doubt, one of the primary objects of the physician to obtain a free passage through the intestinal canal, in yellow fever; and to effect this as gently, and preserve it as steadily, as possible; but we are wholly ignorant that the purgative medicine, here so much extolled, acquired any peculiar reputation, or was peculiarly successful. We should not have descended to a criticism so minute, but that it seems proper (without questioning the author's accuracy of observation) to caution inexperienced readers not to rely too much upon it. The nauseous taste of Glauber's salts, which often, under the most favourable circumstances, offends the stomach, appears to render them particularly objectionable in a disease where that organ is so easily affected, and where vomiting, by whatever means excited, is so difficult to allay, and so pernicious in its effects.—The impropriety of administering emetics is not only admitted, but well stated, by Mr. H. himself, when he comes to consider the course to be pursued after evacuating the bowels, which is, he continues,

"2d. To relax the surface of the body, and induce free perspi-"ration." For this purpose, some, he says, had recourse to emetics, a practice which, as we have before remarked, Mr. H. censures, and in terms that deserve to be quoted. "Vomiting, in "this disease, I have generally observed to be of dangerous ten-"dency. It may, perhaps, not be amiss, when the disease first dis-"covers itself, and is attended with much sickness and vomiting, "to empty the stomach freely, by means of an infusion of cha-"momile flowers; but the exhibition of emetics I consider to be "highly dangerous and improper. I believe I have seen some "cases in which the death of the patient could be ascribed to no "other cause than the incessant vomiting brought on by an eme-"tic given in the commencement of the disease." Of this we have no doubt; melancholy proof having been, more than once, presented to our own eyes; and we hope this fact, so well established, may have some influence in restraining practitioners of medicine from the general administration of emetics, in the beginning of our own country fevers, at least, and emancipate them from a servile acquiescence in the opinions of Dr. Cullen, and some other celebrated European writers.

Of the advantages to be derived from Sudorifics, or from sweating procured by art (to say nothing of the difficulty of effecting it by such means, while the violent action of the arteries is yet

unsubdued) in this disease, so warmly advised by Mr. H. we must be permitted to retain our doubts; not having seen any thing ourselves, or found much in the writings of others, to confirm us in the belief of their utility. Indeed, we much fear that there has been great misapprehension on this subject of sweating; but our limits do not permit us to enter into the discussion at present. We may, however, observe, that profuse sweats have been known to occur in yellow fever, without producing any salutary change in the patient; an event which could scarcely be expected, were Mr. H.'s notions of the consequences of this discharge altogether as just as he seems to suppose.

But though we are cautious how we assent to *all* the means prescribed by Mr. H. we heartily concur with him in recommending the cold bath, or frequent washings of the body with water, or vinegar and water. The practice, however, deserves to be stated with more precision than even Mr. H. has exercised; because, we are well convinced, many and great evils have resulted from an improper and ill-timed application of this remedy, and that it may be employed, if judiciously, in almost every state of the disease, with advantage.

If, in the first days of the fever, cold water be thrown on the patient, we have observed the following effects. First, the pulse was reduced in frequency and force; a general and pleasant coolness was induced over the whole system; and pain was very much mitigated, if not removed. This state of things has continued for some time, greater or less, according to the extent to which the cold affusions were carried, and to other circumstances. Afterward the heat, fever, and pain, gradually returned, till, at length, they became more severe and intolerable than at first. The reason of this no one will be at a loss to conceive who has perused the writings of Brown, Darwin, Hunter, and Rush. Where the like practice has been pursued, towards the close of the disease, we have never remarked any more than a slight and temporary return of warmth. More often the skin has continued cold; and we have been certain that such a torpor of the system has been induced, as has aided the progress of the disease. On the contrary, when, in the first days of the fever, while the heat was great, the pulse strong, and the strength of the body not yet reduced, we have noticed that a gradual application of cool, or even cold water, so that the whole body should be washed, has had very happy effects; especially if persevered in, and frequently repeated. Like beneficial consequences result from similar applications, similarly conducted, toward the close of the disease, if the morbid heat of the skin continue. Nor is it of great importance whether the water be cool or tepid, if the patient be freely exposed to the air. The subsequent evaporation of tepid water probably will not

fall short, in point of benefit to the patient, of the original application of that which is cold. In either case the skin is cleansed of all unpleasant and unsalutary matters; is refreshed by a gentle coolness; the strong pulse of the first stage is diminished in force, the weak pulse of the last stage in frequency; a tendency to sleep is often induced; and perspiration, or sweating, follows as the natural and inevitable consequence of the removal of the morbid heat from the surface of the body. That this is the fact, whether the fever be inflammatory or putrid (to use the ordinary terms of medical writers) is confirmed by the experience of those who have written from a careful attention to facts happening under their own eyes.—But to proceed—When, in the beginning of the fever, the head or the stomach is peculiarly affected, at the same time that the gradual application above mentioned is made to the body *generally*, cold may be *particularly* applied to the suffering part, with great advantage. In this case it should be constant; and very cold water, or ice, is requisite: to the shaved head, if the head be affected; and to the region of the stomach, or by the mouth, if the stomach.

There will sometimes occur, pretty early in the fever, in feeble persons, and where the evacuations have been considerable, a sudden diminution of warmth over the whole body—chilliness, perhaps, and coldness of the lower extremities; while the pain and heat of the head are the same, or are increased. In such cases, warm applications to the feet and legs, with the continuance of cold to the head, have never, according to our observation, failed of good effects.

There is one situation further to be particularized, in which cold applications externally are adviseable. When the inflammatory symptoms of the disease have been obviated by free evacuation, and, as sometimes happens, things seem inclined to remain stationary for a time, we believe that the *shower-bath* may be advantageously employed, if not too long continued. In this case the application of cold has a tendency to accumulate the excitability, or irritability, or sensorial power, by which the skin is rendered more sensible to the impress of the natural stimuli, and more readily resumes its healthy action.

Physicians, in many instances, have persevered in cold applications, in this fever, with too little discrimination. In the last stage of the disease, the whole body, perhaps, except the epigastric region, or the abdomen, will be cold, the head much affected with delirium or coma, and cold applications still persisted in. In this situation of the patient, no benefit, but much injury, must result from their continuance. Wherever morbid heat is observed, there they are proper; but whatever part of the system is much below the standard of healthy heat, requires the careful ap-

plication of warmth. There is nothing peculiar in yellow fever, in this respect, which should overthrow, or cause us to disregard, the general principles which govern us in the treatment of diseases. And it is because some appear to think so, and because the consequences of such a persuasion have been so unfortunate, that we have entered so largely into the consideration of the subject, on the present occasion—as we hope, neither uselessly nor impertinently.

We come now to the *third* indication of cure, which arises when the yellow fever has progressed to what Mr. H. calls the second or putrescent stage of the disease; from which, he observes, few patients recovered.

Without entering into a lengthy examination of particulars, we may observe, that the practice recommended in this stage of the disease could only be successful in its mildest form; nor can we wonder that few patients, who were violently affected, should recover by its means. In respect to blisters, we heartily concur with Mr. H. in recommending their application over the stomach, under the circumstances he mentions; having sometimes seen them arrest vomiting when it had not proceeded to very great lengths; but we must regard them as an equivocal remedy in the advanced stages of the complaint. Perhaps, in cases where there is much congestion, and in the commencement of the disease, after free and repeated evacuations, they might be employed with more success, on the same principle which determines their use in pneumonia. We are at a loss, however, to discover the reason why Mr. H or his instructors, have so wholly passed over and left unemployed the lancet, in a condition of the disease from which, under their common practice, so few recovered; especially when Dr. Rush, and other practitioners of great experience, have borne such ample testimony in favour of its efficacy in restraining those dreadful vomitings that occur in the yellow fever. Equally surprizing is it that no mention is made of calomel and other mercurials, which Mr. Chisholm, Mr. Boag, and the East and West-India physicians in general, represent as nearly a specific in the worst forms of the disease, in those countries; and which Dr. Rush, and others of our countrymen, in Philadelphia, New-York, and New-Haven, have so successfully administered.

We intirely coincide with the author in the caution against the use of wine, bark, &c. in this stage (and all stages) of the fever; and, if we except the addition of spirits, &c. to the patient's drinks and food, agree with him in the general plan of diet.

Under the *last* indication, for restoring strength to the patient, after the solution of the fever, we see little to except against, and much to commend. We have not, it is true, found any particular advantage from bark and wine, even here; but we have ob-

served the best effects from the avoidance of meats, and the sparing use of animal food, in every form. The remarks of Mr. H. on this point, are judicious and just, and prove him to have observed with a degree of accuracy much to his credit.

We have now gone through with our examination of this Essay, on which we have bestowed the more time and attention, as we found some errors the more necessary to be corrected, as they are mingled with interesting truths; and as the subject treated of is of increasing importance.—In respect to the cure of yellow fever, generally speaking, we seem not to have attained, as yet, to that clear, decisive, and prompt method, which shall be always and unequivocally safe and certain. The subject is still embarrassed with doubts and difficulties; and though the disease seems willing enough to afford us opportunities for observing and combatting it, yet, while any uncertainty remains as to the best weapons wherewith to oppose it, the public are much indebted to every person who will step forward and promote discussion concerning them. Nor will the sincere lover of truth, and friend to his fellow men, find any cause for complaint if those which are effectual are contrived, with the overthrow of all his own opinions and speculations, however fondly they may have been cherished and ingeniously devised.

ART. VI. *An Inaugural Dissertation on the Operation of Pestilential Fluids upon the large Intestines, termed by Nosologists Dysentery.* By *William Bay*, Citizen of the State of New-York. New-York. *T. and J. Swords.* 1797. 8vo. pp. 109.

IT is proper to apprize the reader, that the leading principles of this Dissertation are founded on the doctrine of pestilential fluids, which has been taught, for some time past, by the Professor of Chemistry in Columbia College.

In the introduction, Mr. Bay takes notice of an erroneous opinion, too often entertained by physicians, that diseases arising from the exhalations of animal and vegetable substances, during corruption, are putrid. Such persons have not considered the antiputrescent qualities of the vapours emanating from substances undergoing the putrefactive process. Carbonic acid gas, nitric acid, and volatile alkali, always extricated in greater or less quantity during putrefaction, are known to be powerful preventives of corruption. To assert that the products of putrefaction possess a power to putrefy every thing near them, would be just as reasonable as to affirm that the products of combustion set fire to every thing with which they come in contact. Substances hav-

ing undergone combustion are ever afterwards incombustible; and bodies, whose connection has been dissolved by putrefaction, are ever afterwards antiseptic. The well-known experiments of Dr. Alexander leave no doubt on this subject.

This Dissertation is divided into three chapters; the first describes the functions of the alimentary canal; the second treats more particularly of the disease; and the third delivers the doctrine of pestilential fluids, which the author applies to explain the phenomena of dysentery.

On the subject of digestion, the usual arguments are adduced to prove, that trituration, fermentation, and putrefaction, possess no agency in that operation, and that to the solvent powers of the gastric liquor we are to look for the principal cause of the decomposition, which aliment undergoes in the stomach. The experiments of Spallanzani, Reaumur, Hunter, and Steevens, so far as is necessary to establish this point, are briefly stated.

The properties and use of the bile next engage our author's attention. Having exhibited a short view of the structure and functions of the liver, he proceeds to describe the ordinary appearances of the bile, and the products it yields on being subjected to distillation. These are stated, on the authority of Fourcroy, to consist of "a yellow alkaline phlegm, an empyreumatic oil, a "considerable quantity of the carbonate of ammonia, an elastic "fluid united with carbonic and hydrogene gas, and the carbonate "of soda." Rejecting the opinion of the agency of bile in aid of digestion, by means of its saponaceous, blending qualities, he concludes, "that the principal use of it in the animal economy, "is to prevent diseases arising from the putrefaction of animal "and vegetable matter." Supposing the spontaneous decomposition of these matters to yield nitric acid or azotic oxyd, which he considers as the parents of a multitude of diseases, he thinks the alkaline qualities of the bile well calculated to neutralize and destroy them. He asserts, that the blame thrown on the bile, as tending to become acrimonious and putrid, and thereby generating the summer and autumnal diseases, is altogether unfounded; and that, on the contrary, the large quantity of it produced in the diseases called bilious, is a bountiful provision of nature to counteract the pernicious matter which forms the real cause of such fevers.

That putrefaction takes place in the feculent matter of the large intestines, Mr. Bay concludes from the existence in these tubes of all the circumstances necessary to produce this effect, viz. air, heat, moisture, the matters most apt to be wrought upon by such causes, and the slowness of motion requisite for their full operation; and also from the quantity and quality of gazeous fluids emitted from the rectum. Animal diet must, he thinks, greatly increase the mischief of this decomposition, as the lean part con-

tains a large proportion of azote, and no small quantity of oxygene, the two principles from whose union he apprehends so much danger. The fat part, on the other hand, chiefly consisting of carbone and hydrogene, seems to be innocent.

From a chemical analysis of the fæces, it appears, our author informs us, that they emit *hydrogene gas*, *sulphurated hydrogene gas*, *phosphorated hydrogene gas*, *carbonic acid gas*, and *azotic gas*. None of these gases, nor any combination of them yet known, produce mischievous effects. Hydrogene gas alone is perfectly innocent: combined with carbone, it forms fat; with oxygene, water; with azote, ammonia. Carbonic acid gas and azotic gas constantly exist in the human constitution, and, in moderate quantity, are found to be intirely harmless.

Having found these gases insufficient to produce dysentery, Mr. B. concludes that a chemical combination of azote and oxygene, forming, according to the degree of oxygenation, either an oxyd of azote, or the nitric acid, constitutes the morbid cause in question. He supposes this to be rendered probable by the animal origin of the nitric acid—by the large quantity of animal matter taken in as food—by the use of the fæces in the formation of nitre—by Pringle's observation of an acid existing in the fæces—by the increase of the fœtor of feculent matter, from the addition of nitric acid, as observed by Pringle—and by Homberg's fact of a salt, discovered in the fæces of a man fed upon coarse bread, which, when exposed to fire, detonated like nitre.

The noxious fluid, thus formed by a chemical union of azote and oxygene, our author asserts, may be either generated in the large intestines, and immediately produce dysentery by coming in contact with their coats, or arising from the immense masses of putrefaction, which overspread so large a portion of the earth, in the form of vapour, it may be introduced into the bodies of men by the lungs, by the stomach, or by the pores of the skin, and, in this manner, produce the same disease. This difference, as to the source of the morbid cause, will readily account for the sporadic, endemic and epidemic appearances of dysentery.

From the concurring testimony of a number of the most eminent medical writers, the connection of this disease with remittent and intermittent fevers, is easily evinced; and hence their origin from the same cause may be inferred with great probability.

The means of preventing the disease next employ our author's attention; and these he refers to three heads. 1. He enjoins abstinence from all animal food, except its oils and fats, whenever the disease is prevalent; 2. He directs to avoid costiveness; and, 3. He orders removal from an infected atmosphere.

The method of cure is distributed into the three following indications—"1st. To remove septic and feculent matter from the

alimentary canal: 2d. To correct the vitiated state of the atmosphere, or to remove from it: 3d. To allay the inflammation, and the other injuries, which the alimentary canal may have sustained by the original cause of the disease, and the remedies made use of to effect a cure."

The first indication is promoted by freely evacuating the intestines. For this purpose the author recommends carbonate of pot-ash (salt of wormwood), acetite of pot-ash (regenerated tartar), sulphite of pot-ash (vitriolated tartar), tartrite of pot-ash (soluble tartar), tartrite of soda (Rochelle salt). As these salts are all decompounded by the nitric acid, their alkaline bases will serve to neutralize it. The muriate of soda (common sea salt) united with the vegetable acids, has been also much commended. Carbonate of pot-ash is directed to be given in doses of four grains, every two or three hours; and it is advised to combine it with jalap or rhubarb, when these are chosen as cathartics. Oleum ricini and the cerated glass of antimony are also recommended; and to relieve the tenesmus, starch-clysters, with tincture of opium, are directed to be injected.

For the purpose of cleansing the body externally, and, at the same time, reducing the temperature when too much heated, as well as promoting a diaphoresis, the cold bath is ordered to be employed.

To correct the vitiated atmosphere surrounding the patient, the chamber is directed to be ventilated and frequently changed, the floors to be cleaned with alkaline ley, and the walls to be white-washed with lime. The bed-clothes and body-linen of the sick should also be often changed. And if by these means it be not possible to correct the vitiated air, it will become necessary to remove the patient to a healthier situation.

To accomplish the third indication, blood-letting is principally relied on, which is advised to be small, and to be frequently repeated.

The diet of the sick is ordered to consist of soups, milk, and vegetable substances: the lean parts of animals are forbidden. And, at the close of the disease, the usual corroborant remedies, such as bitters and the Peruvian bark, are recommended.*

We have thus drawn a brief outline of this Dissertation, which

* Mr. B. has omitted to recommend mercury in his plan of treating dysentery. In the more severe or protracted cases of the disease, when the ordinary remedies have failed, and when violent degrees of inflammation or ulceration of the larger intestines are either threatened or supposed actually to have taken place, we know no remedy so much to be relied on as calomel, combined with small quantities of opium, till it gently touches the mouth, in the manner directed by Dr. Clark. *See Observations on the Diseases which prevail in long Voyages to hot Climates*, p. 340.

contains matter deserving the serious consideration of every practitioner of medicine. We regret that our limits do not permit us to notice much of the reasoning and illustration employed by Mr. B. to vindicate and confirm his principles.

But as many doubts are still entertained concerning the doctrine maintained in this essay, it now becomes our duty to state these with all possible impartiality.

The acknowledged connection of dysentery with remittent and intermittent fevers, while it favours the opinion of their origin from a common source, has been thought to expose Mr. B.'s hypothesis of the generation of the former disease in the large intestines, to an obvious objection. In these fevers it commonly happens that bile is secreted and poured into the alimentary canal in large quantity; but this fluid, from its strong alkaline property, is the direct neutralizer and extinguisher of the noxious matter supposed to produce dysentery. How happens it then, that, in such circumstances, the system should manifest such tendency to fall into dysentery, notwithstanding the immediate presence and operation of so large a quantity of its direct antidote? In jaundice, when the fæces are deprived of the neutralizing benefit of the bile, and when costiveness often detains them a long time in the large intestines, exposed to all the hazard of the evolution of this poison, dysentery is not observed to take place.

The infrequency of sporadic dysentery has been also made a ground of objection to this doctrine. The circumstances leading to putrefaction must be supposed always, more or less, to exist in the large intestines, viz. heat, moisture, air, and a quantity of animal and vegetable matter. The degree of heat in the intestines may be considered as nearly at the same point in all seasons and in all climates. Indigestion, and other causes weakening the assimilating powers of the alimentary canal, and giving occasion to the evolution of such noxious matters as the process of corruption is apt to generate, often occur. Sporadic dysentery, from this internal poison, on these principles, should be frequently expected; and yet it is seldom observed, except where season, climate, particular states of the air, or other similar causes exist, which are likely to furnish the noxious matter that appears to gain admittance, from without, into the system.

It has been likewise objected to Mr. B.'s doctrine, that various cathartic remedies, and particularly the sulphates of soda and magnesia (Glauber's and Epsom salts) have, from actual experience, produced equal, if not superior, good effects in dysentery, to the saline articles that are decompounded by the nitric acid, and, on that account, according to his principles, ought to operate in a much more powerful manner.

In opposition to the theory supported in this essay stand the experiments of Dr. Carmichael Smyth, who, on board the Union hospital-ship, at Sheerness, found the fumes of nitrous acid a successful means not only of destroying the offensive smell of putrid animal effluvia, but also of extinguishing contagion.

On ground not very dissimilar rests an objection drawn from the employment of nitrous acid, by Mr. Scott and others, as a remedy in hepatic and other diseases of the abdominal viscera, and in syphilitic affections. From these cases it appears that this active substance has been daily taken, in considerable quantity, into the stomach, for some length of time, and generally with great advantage.

"The nitrat of silver, or lunar caustic, is mentioned by Mr. Hahnemann as a most effectual antiseptic: he says, that flesh laid in a solution of one part of this nitrat in five hundred of water, will not putrefy; and if, after being kept a fortnight in a strong solution, the flesh be taken out, and laid in a warm place, it will be gradually dried, without contracting the least disagreeable smell, will become very hard, and will never breed maggots. He asserts also, that water, in which a very small proportion of this nitrat is dissolved, will remain perfectly sweet, provided it be kept from sunshine, which would cause the nitrat to precipitate. The water is perfectly innocent, may be used as a common drink, and may be peculiarly advantageous at sea, as a preservative against the scurvy." Appendix to Monthly Review, vol. xiv.

The engraver, whose etching on copper is performed by means of nitrous acid, must be greatly exposed to inhale, or otherwise receive, the nitrous gas evaporating from the plate: yet we have never understood that these artists experience any thing like pestilential injury from breathing over this vapour. Silver-smiths, and some other workers in metals, who employ aqua-fortis in their operations, present facts of similar import.

How far these experiments and facts should be admitted to invalidate the theory delivered in this Dissertation, we shall not undertake to decide, but shall willingly submit them to the test of further time and experience. Whatever variety of opinion or discordancy of facts may exist on this subject, we are persuaded the most perfect unanimity will prevail in considering the principles here delivered by Mr. B. and supported by so many powerful arguments, as highly interesting to the science of medicine and to the welfare of mankind. And we are convinced the same unanimity will be felt in ascribing to our author the praise of diligence, ingenuity, and a comprehensive survey of the subject on which he treats. This early fruit of his studies, worthy of a more advanced age and mature experience, leaves us no room to doubt that his future professional career will be honourable to himself, and useful to his country.

ART. VII. *An Inaugural Dissertation on that Grade of the Intestinal State of Fever known by the Name of Dysentery.* By *James Fisher*, of Delaware, *&c.* Philadelphia. *Ormrod and Conrad.* 8vo. pp. 52. 1797.

THE Essay before us exhibits honourable testimony in favour of the ingenuity and industry of the author. It may be questioned, however, whether his speculations have contributed any thing to the illustration of the nature of the disease, or the treatment recommended by him to its cure, of which the public was not already possessed. Still, as many sensible remarks and judicious directions are here collected, in a compact form, it may not be an unpleasing nor unuseful task, especially to the young physician, to refresh his memory by the perusal of Mr. Fisher's Dissertation. But, when we have acknowledged that, in this point of view, we have derived some pleasure from the reading, and, on the whole, consider this performance as creditable to the writer, we may be allowed to offer a few suggestions to him, as matter of future reflection and inquiry, in the course of the practical application of those studies the collegiate pursuit of which he has just concluded.

With respect to what is said concerning the dysentery being only a *febris introversa*, it may be remarked, that the general line of argument pursued here, would reach to almost every disease not absolutely local, and comprehend the most opposite states of the system. And if it be insisted that this is the fact, and that all general diseases are fevers of various forms and degrees, we have yet to discover the practical utility of such an opinion. The necessity will recur as strong as ever for particular description and careful discrimination. This being the case, it would, perhaps, be more useful and more accurate to say, that the state of the bowels which we call dysentery, is accompanied by fever, or that fever constitutes one of the essential parts of the disease. Even then we should not have a very distinct conception of the fact without some further and more precise account of the nature of the fever.

So, in regard to what Mr. F. considers as the remote causes of dysentery, viz. marsh-miasmata and contagion, it may admit of doubt whether he has viewed the subject with philosophic accuracy—whether unity of effect does not bespeak unity of cause—whether there is any specific difference between the infectious material which rises from marshes, and that generated in the intestines—and whether this material, or principle, or whatever it may be denominated, be not rather the *exciting* than the *remote* cause of dysentery.

The old notion of the mischiefs arising from the checkage of perspiration, that yet surviving offspring or progenitor of the humoral pathology, should not have found its way into a modern Dissertation, in which effects ought to be more carefully distinguished from causes.

But these are, comparatively, slight defects, and after all depend much on opinion. In the more material part, the Cure, Mr. F. has carefully collected and arranged the various remedies whose efficacy has been ascertained by the experience of his predecessors, with brief and general rules for their exhibition, it is true, and such, perhaps, as future practice may teach him somewhat to vary, but more correct and particular than are often to be found in Inaugural Essays.

ART. VIII. *An Inaugural Dissertation on the Dysentery.* By *Colin M'Kenzie*, of Baltimore, *&c.* Philadelphia. *Ormrod and Conrad.* 8vo. pp. 47. 1797.

MR. M'KENZIE pursues nearly the same course with his fellow-student, Mr. Fisher. We often recognize not merely the same opinions, but almost the same words, in either essay. These resemblances have probably arisen from attending the instructions of the same teachers, and from frequent interchange of sentiments with each other. The mode of cure proposed by Mr. M. does not differ materially from that laid down by Mr. Fisher.—On the whole, we think Mr. Fisher's Dissertation deserves the preference.

ART. IX. *An Inaugural Dissertation on Hydrocele.* By *Samuel Jones*, A. M. *&c.* Philadelphia. *Ustick.* 8vo. pp. 27. 1797.

THIS is a short, but comprehensive and well-written Essay. We intirely agree with Mr. Jones in the opinion that Mr. Earle's method of effecting the radical cure in hydrocele, by *injection*, is preferable to any other yet proposed.

ART. X. *An Inaugural Dissertation on the Rheumatic State of Fever.* By *Edward North*, of South-Carolina, &c. Philadelphia. *Woodward.* 8vo. pp. 37. 1797.

IT is sufficient to give the title of this Essay, which, relating to a very common disease, and presenting no new views of the subject, will probably never be consulted by the practitioner or the theorist.

ART. XI. *An Inaugural Essay on Dropsy, or the Hydropic State of Fever.* By *William Allston*, of George-Town (S. C.) &c. Philadelphia. *Woodward.* 8vo. pp. 60. 1797.

MR. ALLSTON adopts the theory of Dr. Rush in respect to dropsy—a theory which those of our readers who may not already be acquainted with it, will find ably developed in vol. ii. of that gentleman's *Medical Observations and Inquiries.* To the merit of originality, therefore, the author of the present Essay can prefer no claim; but we may remark that he has multiplied authorities, in favour of his hypothesis, in a manner which displays considerable reading and reflection. Those who are accustomed to Dr. Rush's manner of treating a subject, will observe, that the pupil has preserved, with great fidelity, both the method and turn of expression of the preceptor. This circumstance may be a further recommendation of Mr. A.'s Dissertation to notice, which, from its general merit, it well deserves.

METEOROLOGICAL OBSERVATIONS *for July*, 1797, *made by* GARDINER BAKER, *in the Cupola of the Exchange in the City of* New-York.

Days of the Mon.	Thermom. observed at Sun-rise.	3 P. M.	Prevail. winds. 5 M.	3 E.	Clear.	Cloudy.	Barometer observed at Sun-rise.	3 P. M.
1	69	77	S W	S	2		29 72	29 62
2	72	89	S W	S W	1	1	29 50	29 45
3	73	80	S	S	2		29 50	29 54
4	70	82	W	W	2		29 58	29 70
5	64	84	N W	N W	2		29 75	29 76
6	67	85	S W	S W	2		29 79	29 78
7	70	76	S	S		2	29 73	29 65
8	70	73	S E	S E		2	29 55	29 42
9	71	85	S	N E	1	1	29 57	29 40
10	70	89	N	S	2		29 50	29 55
11	70	84	E	S	1	1	29 54	29 58
12	68	83	N	N W	2		29 67	29 68
13	72	85	N	S W	2		29 70	29 75
14	71	82	S	S	2		29 77	29 73
15	72	85	S W	S	1	1	29 69	29 60
16	70	85	N W	W	2		29 56	29 56
17	73	77	S E	S E		2	29 55	29 60
18	70	81	N E	S E	2		29 67	29 73
19	69	88	N W	N E	2		29 71	29 72
20	74	90	N E	S	2		29 75	29 78
21	76	90	W	S	2		29 81	29 81
22	73	85	W	S	1	1	29 78	29 75
23	74	80	S	S	1	1	29 65	29 63
24	71	83	E	S E	2		29 53	29 46
25	74	74	S	N	2		29 34	29 35
26	70	86	N W	N W	2		29 48	29 58
27	73	81	S W	S	1	1	29 70	29 73
28	69	80	E	N E	1	1	29 70	29 69
29	70	82	N E	E	2		29 69	29 71
30	71	80	E	S W	2		29 72	29 72
31	71	77	E	S E	1	1	29 72	29 72

Results of Meteorological Observations for July, 1797.

Mean temperature of the Thermometer at sun-rise, deg. 70 8 hund.
Do. do. of the do. at 3 P. M. 82 5
Do. do. of the do. for the whole month, 76 6
Greatest monthly range, between the 6th and 21st, 26 0
Do. do. in 24 hours, on the 10th, 19 0
Six days it rained, and a considerable quantity has fallen.
Five days it thundered and lightned, but not in great abundance.
Coldest day the 5th. Warmest day the 21st.

METEOROLOGICAL OBSERVATIONS *for August*, 1797.

Days of the Mon.	Thermom. observed at Sun-rise.	2 P. M.	Prevail. winds. 5 M.	2 E.	Clear.	Cloudy	Barometer observed at Sun-rise.	2 P. M.
1	70	77	E	N E		2	29 60	29 61
2	70	75	E	N E		2	29 65	29 72
3	70	73	E	E		2	29 77	29 79
4	70	71	E	E		2	29 76	29 70
5	75	80	S E	S E	1	1	29 51	29 68
6	71	78	W	N W	1	1	29 71	29 68
7	70	79	N	N	1	1	29 68	29 73
8	71	73	E	S E		2	29 60	29 42
9	69	81	S W	S W	2		29 45	29 51
10	65	78	N W	W	2		29 76	29 82
11	65	76	N E	S	2		30 08	29 90
12	67	75	S	S	1	1	29 87	29 75
13	73	81	S	S W	1	1	29 50	29 45
14	65	74	W	N W	2		29 53	29 59
15	56	76	N W	W	2		29 80	29 80
16	62	77	-		2		29 87	29 88
17	65	78	S W	S	2		29 90	29 84
18	70	79	S	S	1	1	29 69	29 60
19	68	81	S	S W	2		29 35	29 32
20	68	81	W	S W	2		29 33	29 41
21	67	77	N E	S W	1	1	29 51	29 53
22	71	71	E	E		2	29 57	29 60
23	66	76	N E	N E	2		29 58	29 53
24	67	82	S W	S W	2		29 54	29 54
25	68	77	E	S E	2		29 54	29 58
26	72	78	S	S	1	1	29 72	29 76
27	66	76	N W	S W	2		29 52	29 49
28	66	78	N W	N W	2		29 57	29 57
29	61	74	N W	W	2		29 70	29 70
30	64	71	E	S E		2	29 75	29 78
31	67	75	E	S E		2	29 80	29 80

Results of Meteorological Observations for August, 1797.

Mean temperature of the Thermometer at funrife, deg. 67 5 hund.
Do. do. of the do. at 2 P. M. 78 6
Do. do. for the whole month 72 5
Greateft monthly range, between the 15th and 24th, 26 0
Do. do. in 24 hours, on the 15th, 20 0
Twelve days it rained, and a very large quantity has fallen.
Seven days it thundered and lightned, and there has been a conſiderable quantity.
Coldeſt day the 15th. Warmeſt day the 24th.

METEOROLOGICAL OBSERVATIONS *for September*, 1797.

Days of the Mon.	Thermom. observed at Sun-rise.	Thermom. observed at 2 P. M.	Prevailing winds. 6 M.	Prevailing winds. 2 E.	Clear.	Cloudy	Barometer observed at Sun-rise.	Barometer observed at 2 P. M.
1	73	84	S W	S W	1	1	29 72	29 70
2	76	87	S W	W	2		29 51	29 53
3	63	76	N W	N W	1	1	29 60	29 62
4	63	73	W	N W	2		29 65	29 72
5	61	79	W	S W	2		29 73	29 70
6	58	66	N E	E	2		29 80	29 80
7	58	66	N E	N	2		29 88	29 88
8	57	70	N E	E	2		29 79	29 73
9	58	71	N E	N	1	1	29 82	29 86
10	59	71	N E	N E	2		29 91	29 86
11	52	70	N E	N E	2		29 79	29 75
12	59	73	E	N	2		29 70	29 65
13	60	72	S W	S W		2	29 67	29 65
14	63	80	S W	S	2		29 60	29 59
15	68	71	S W	S W		2	29 54	29 48
16	64	74	N	N	2		29 56	29 56
17	56	70	N E	W	2		29 60	29 59
18	58	64	E	S E	1	1	29 55	29 40
19	57	64	W	N W	2		29 27	29 41
20	51	66	W	W	2		29 70	29 70
21	56	69	S E	S E	1	1	29 70	29 70
22	59	67	N E	E	2		29 70	29 70
23	57	72	N W	E	2		29 64	29 60
24	56	75	W	W	2		29 55	29 45
25	58	74	W	N W	2		29 40	29 50
26	57	69	E	W	1	1	29 60	29 50
27	46	62	N	N W	2		29 80	29 80
28	53	71	N W	N W	2		29 80	29 80
29	58	78	S W	S W	2		29 79	29 81
30	60	65	N E	E		2	29 97	30

Results of Meteorological Observations for September, 1797.

Mean temperature of the Thermometer at sun-rise,	deg	59	2 hund.
Do. do. of the do. at 2 P. M.		71	6
Do. do. for the whole month,		65	4
Greatest monthly range, between the 2d and 20th,		26	0
Do. do. in 24 hours on the 29th.		20	0

Four days it rained, and a very small quantity has fallen.
Coldest day the 20th. Warmest day the 2d.

A TABLE *of Patients admitted into the New-York Hospital, from the 1st of July to the 1st of October, 1797; shewing the Disease for which each was received, with the Event of the Case.*

MEDICAL *Patients admitted in July.*

DISEASES.	No.	Cured.	Relieved.	Died.	Remain under Care.	Result.	
Amenorrhea	1				1		
Ascites	3	2			1 elop.		
Diarrhœa	1	1					
Dysentery	1	1					
Dyspepsia	3	1		1	1		
Gravel	2		1	1			
Hepatitis	1	1					
Intermitting Fever	4	3		1			
Mania	3				3		
Pulmonary Consumption	3		1		1		
Rheumatism	4	2			2		
Syphilis	6	3			3	Received	43
SURGICAL.							
Contusion	3	1			1 & 1e.	Cured	17
Fracture	1				1	Relieved	2
Scrofula	1				1	Died	3
Ulcer of the Leg	5	2			3	Eloped	2
Wounded	1				1	Discharged	1
						Remain	18
							—43

August. MEDICAL.

Amenorrhea	1	1				
Anasarca	1				1	
Ascites	3	1		1	1	
Diarrhœa	2	1			1	
Dysentery	3	2		1		
Dyspepsia	1	1				
Gonorrhœa	3	1			2	
Herpes	1	1				
Intermittent Fever	11	9			2	
Mania	4				4	
Pulmonary Consumption	1				1	
Opthalmia	1	1				
Rheumatism	3	1			2	
Syphilis	6	2			3 & 1e.	

SURGICAL.

DISEASES.	No.	Cured.	Reliev.	Died.	Remain under Care.	Result.	
Contusion	4	2			2	Received	66
Fracture	2	1			1		—
Scrofula	1				1	Cured	33
Syphilis	10	5			4&1e.	Died	2
Ulcer of the Leg	7	3			4	Eloped	2
Wounded	1	1				Remain	29
							—66

September. MEDICAL.

Anasarca	1		1				
Ascites	1				1		
Debility	1				1		
Diarrhœa	1	1					
Dysentery	2	2					
Dyspepsia	1	1					
Gonorrhœa	1				1		
Intermittents	12	7			5		
Mania	7	1			6		
Opthalmia	1	1					
Pulmonary Consumption	1				1		
Rheumatism	6	2			4		
Syphilis	3	2			1		
Typhus	5	1		1	3		

SURGICAL.

Diseased Hip-Joint	1				1	Received	60
Fistula Lachrymalis	1				1		—
Scrofula	1				1	Cured	26
Syphilis	6	3			3	Relieved	1
Ulcer of the Leg	5	4			1	Died	1
White Swelling	2				2	Remain	32
Wounded	1	1					—60

SUMMARY.

Patients received into the New-York Hospital from the first of July to the first of October, 1797, exclusive of those then remaining under care, 169

Of this number have been *cured*, ... 76
relieved, ... 3
died, ... 6
eloped 4
was *discharged*, ... 1
remaining under *care*, ... 79
—169

A RETURN *of Patients admitted to the Care of the New-York City Dispensary, from the first of July to the first of October*, 1797.

JULY.

DISEASES.	No.	Cured	Relieved.	Died.	Removed to the Hospital.	Result.	
Amenorrhœa	1	1				Received	54
Asthma	2	2					
Hectic Fever	1	1				Cured	50
Erisipelas	1	1				Relieved	3
Syphilis	5	5				Died	1
Dyspepsia	3	3					
Diarrhœa	14	12	2				54
Jaundice	1	1					
Pneumony	3	3					
Cough	2	1	1				
Pulmonary Consumption	1			1			
Hooping-cough	2	2					
Remittent Fever	3	3					
Fracture	1	1					
Sore Legs	3	3					
Quincy	1	1					
Opthalmia	2	2					
Hæmoptisis	1	1					
Worms	1	1					
Abscess	1	1					
Cholera	3	3					
Herpes	1	1					
Dysentery	1	1					

AUGUST.

Pneumony	4	4				
Intermitting Fever	1	1				
Syphilis	4	3			1	
Typhus	1	1				
Remitting Fever	3	2			1 Elop.	
Cholera	3	3				
Pulmonary Consumption	3	2		1		
Diarrhœa	3	3				
Menorrhagia	1	1				
Constipation	1	1				
Head-ach	1	1				
Sore Legs	3	2	1			
Indigestion	1	1				
Dysentery	7	7				
Calculus	1	1				

AUGUST continued.

DISEASES.	No.	Cured.	Relieved.	Dead.	Removed to the Hospital.	Result	
Opthalmia	1	1				Received	46
Abscess	1	1					—
Sphacelus	1	1				Cured	41
Hectic Fever	2	1		1		Relieved	1
Epilepsy	1	1				Died	2
Burns	1	1				Hospital	1
Worms	1	1				Eloped	1
Colic	1	1					—46

SEPTEMBER.

Contusion	2	2				Received	32
Anasarca	3	2			1 u. care		—
Indigestion	2	2				Cured	26
Scrophula	1	1				Relieved	5
Dysentery	3	3				Under care	1
Bilious Remittent Fever	3	3					—
Rheumatism	5		5				32
Pneumony	2	2					
Worms	5	5					
Syphilis	1	1					
Opthalmia	1	1					
Intermittent Fever	1	1					
Diarrhœa	2	2					
Bilious Colic	1	1					

SUMMARY.

Whole number of Patients admittted into the New-York City Dispensary from the first of July to the first of October, 1797, 132

Of this number have been *cured*, ...	117
relieved, ...	9
died, ...	4
eloped, ...	1
remains under *care*, ...	1
	132

HUGH M'LEAN.

MEDICAL NEWS.

MEDICAL NEWS.

DOMESTIC.

IN the course of the late and present seasons the Yellow Fever has been epidemic, with considerable mortality, in Bristol and Providence, State of Rhode-Island; in Philadelphia; in Baltimore; and in Norfolk, Virginia. Reports have circulated, but we know not with what foundation, that it was also prevalent in Charleston, South-Carolina. We are happy to have it in our power to communicate certain information, that this disease has wholly ceased its ravages at Bristol and Providence; that there is every appearance of its speedy departure from Philadelphia and Baltimore; and that at Norfolk (where the disease appears to have originated indisputably in local cases) the mortality is at an end. The Remitting Fever has renewed its attacks, but without any signal mortality, in Sheffield, Massachusetts, and New-Milford, Connecticut; and the Dysentery has been very rife in Hanover, New-Hampshire. The following article, from the last-mentioned place, we have reason to believe correct; but more particular information is desirable.—

"Hanover, September 9, 1797. The following is an accurate "statement of the number of inhabitants, and the late mortality, "in the vicinity of Dartmouth College:—

"The whole number of inhabitants, including members "of College, amounts to about	520
"Number of people affected with the Dysentery, about	203
"Died of Dysentery, only	5
"Of Dysentery and Whooping-Cough,	7
"Of Consumption and Dysentery,	4
"Without Dysentery,	4
"Whole number of deaths,	20

"Of these five were adults, the other fifteen were children. The "ravages made by the late sickness are the more remarkable, as "the place, generally, heretofore, has been esteemed one of the "most healthy places in the world. This is the first instance of "the prevalence of an epidemic."

A remarkable disease occurred, during the last spring, among the CATS in London, which extended itself over several other parts of England. In three parishes in London, upwards of 5000 cats are computed to have been destroyed by it, within a fortnight. The most common period of the disorder is said to have been six or seven days; and a puking of greenish bile, and delirium, are enumerated among the symptoms, of which we have seen no account sufficiently accurate to be relied on. The following paragraph is extracted from the "Account of Diseases in London, from the 20th of March to the 20th of April, 1797"—published in the Monthly Magazine for April last.

"After the abatement of the Epidemic Catarrh, a violent dis-"ease fell upon the Cats, by which many hundreds of those do-"mestic animals perished. The particular symptoms of their "malady I had not a sufficient opportunity of ascertaining: but "from dissections, it appears, that the bowels had been drawn to-"gether by a violent cramp, or spasmodic constriction, involving "likewise the omentum, and preventing any passage. In some "the intestines were partially red, or blackish; but in the greater "number of instances, the contraction and extraordinary twisting "of the bowels had taken place without any marks of inflamma-"tion."

This is probably the same disease which has appeared, among the same animals, in various parts of the United States, the present summer and autumn. Hitherto, though we have taken some pains to this end, we have been unable to trace the exact progress, or determine the precise symptoms of the disease. In the hope of obtaining communications from our correspondents and others, we have collected together such information as has already been made public, and as has fallen under our more immediate observation.

The *cat-distemper* made its appearance in Philadelphia in the latter end of May or beginning of June last; and is supposed to have destroyed not less than four or five thousand cats.

In August it was observed among the cats in New-York.—In general, the animal appeared moping and melancholy, without appetite, thirsty, restless, and inclined to solitude. Numbers died, after a gradual decline, in this manner. Scarcely one was to be seen but that looked thin and weak. In other instances the animals were seized with puking and delirium; and when delirious, flew about the houses or rooms they were in, but without attempting to injure any person. Some were seized with delirium and puking only at the last of the disease. Some were observed to have an uncommon flow of saliva. We know not that it was observed whether such recovered.

During the continuance of this epidemic in New-York, a num-

ber of these animals left the city, and took refuge in the adjoining country houses. Three arrived, in one day, at the house of a gentleman who resides near Corlaer's Hook, about two miles from town.

The number that died we have not accurately learned; no estimation makes it less than three or four thousand.

In two instances, *lap-dogs*, that were wont to play with the *cats*, in the houses to which they belonged, were affected with a similar complaint. One was killed, near to death: we are uninformed of the fate of the other.

The next public intimation of the appearance of the *cat-distemper* was from Portsmouth, New-Hampshire, (if we recollect rightly) towards the end of August.

Early in September its arrival in Boston was announced in the papers of that town.

The last account we hear of it is from Albany, in the beginning of October.

None of the editors of the public papers, in either of these places, as far as we can discover, have given any further information concerning this disease, than merely to notice its existence, and the mortality occasioned by it.

In a Meteorological Journal, accompanied by remarks on the diseases of the season, and their treatment, which has been kindly transmitted to us by that curious and careful observer, Dr. Shadrack Ricketson, of Dutchess County, we find the following remarks, towards the close of his observations on the three spring months. "It may not be wholly foreign or improper to remark, "that *horses* were very generally and severely afflicted this spring "(1797) with the common distemper in the glands of the head "and throat; for, it has frequently been observed, that horses "and dogs have been affected as concomitant, or presaging, epi- "demic or pestilential diseases: but whether the present, or some "future sickness is to follow, I leave for future observation to "determine."—In concluding his remarks on the weather and diseases of the last summer, Dr. Ricketson says, "In my last I "mentioned that horses were severely afflicted with their common "disorder, which has continued to be the case, but not so gene- "rally; and, I may now add, that *dogs* have been, in like manner, "violently disordered with a cough, &c. most of the summer."

Information has been conveyed to us, from a part of Connecticut, of some distemper among *neat cattle*; but the imperfect statement of facts which we have received, and the expectation of more minute accounts, induce us to do nothing more at present than to mention the fact.

A Richmond (Virginia) paper, of the date of August 16, 1797, has the following paragraph. "For upwards of a week past, the "inhabitants of this place have been astonished with the appear- "ance of vast quantities of dead *fishes*, which have been continually "floating down James' River." Various unsatisfactory conjectures are offered as to the cause of this singular mortality.

We are informed that a remarkable mortality, we know not from what cause, happened in the summer of 1795, among the Fish in Saratoga Lake, in this State, (at which time it was noticed that the water in the lake had a remarkable greenish appearance, with a thick skim on its surface): and a like pestilential disease is said to have destroyed great numbers, some years ago, in a pond near, or in, the town of Brookfield, State of Massachusetts.

As connected with these facts, we insert an extract of a letter from Mr. George Harries, to Dr. Mitchill (then at Edinburgh) dated Haverford-west, Pembrokeshire, South-Wales, July 22d, 1786.

"As Edinburgh must now be very thin, and those who remain, "most likely are engaged in little societies for the improvement "and entertainment of themselves, during this idle time of the "year, every *true* fact will probably be acceptable.—That which "I am going to mention relates to the poisonous effects of tobac- "co, and may be relied on as an undoubted one.—An American "ship, belonging, I think, to the Tracey's, of Boston, was un- "fortunately lost, through the ignorance of the pilot, on our "rocks. Her cargo consisted of tobacco, which floated in large "quantities on the surface of the water, and impregnated it with "its flavour to a considerable extent. It proved a deadly poison "to great numbers of *fish;* many of which were taken up and "thrown upon the shore in the neighbourhood of the wreck: it "is not material what fish:—lobsters and flat-fish were among the "number."

Bennington, (Vermont) August 3, 1797. A quantity of purging salts, manufactured from a spring in *Orwell*, in this State, has been exhibited to the District Medical Society, which are found to be equal, if not superior, to any of the imported salts: they are less nauseous, and are milder in their operation.

The following article, which appeared in a Boston paper last August, has excited much unsatisfactory observation and inquiry.

"On the 16th inst. Mr. Merrick, who resides at Little-Cam- "bridge, discovered a comet, covering the star placed at the in- "sertion of the tail in the body of the Little Bear. It was be- "tween 8 and 9 o'clock in the evening; and at the same hour on "the 17th inst. he saw it advancing towards the star *(mu)* in the

"Dragon, of which it had crossed the body; having moved at a "rate through the heavens twice as rapid as the moon. On the "18th and 19th inst. it will be nearly on a line with what may be "called the pointers of the Little Bear; and about eight or nine "in the evenings of those days, if the weather be clear, it will be "seen to the west of our zenith, and not very far distant from "it. This comet at present has no tail, but appears as a small ne-"bulous or cloudy star, of a round form, being most bright to-"wards its centre or nucleus."

Since the publication of the above paragraph, the comet has been mentioned in the public papers, from different parts of the continent, as having been seen in the most opposite situations, and at irreconcilable hours in the evening. We, ourselves, have had opportunity to observe, that both the planet *Venus* and the planet *Jupiter* have been mistaken for the comet. And as the planet Mercury was in its greatest elongation on or about the 19th of September, (when the reports concerning the comet were most prevalent) it is not impossible that some of the observers may have had a glimpse of this star, a little after sun-set, and have mistaken it also for a comet.—As no new observations have been published by the gentleman who is represented as having originally made the discovery, the public have lately doubted the reality of any such appearance. It would be doing a real service to science (if the whole of what has been said about the comet be not a mistake) to publish such correct information that there may be no future misapprehensions on the subject, and that, if it be not too late, observations may be multiplied concerning it, in various parts of the United States.

The Chemical Museum of Columbia College, New-York, has been lately enriched by an extensive collection of MINERALS, the donation of Mr. Samuel Owens, a gentleman from Belfast (Ireland,) who has become a citizen of this state.

There is in the city of New-York, at this time, an instance of a singular monster. A calf, about seven or eight months old, is now exhibited as a shew, whose *heart* is apparently in its neck or throat, a little forward of the brisket. Its pulsations can be seen at a considerable distance, and its motions, like those of the auricles and ventricles, very plainly felt. The best judges in the city, who have examined this remarkable creature, are of opinion, unanimously, that this lump, or tumor, is not an *aneurism*, *even*, nor any thing of those kinds, but truly the *heart*. The calf appears somewhat lean, but lively, and in good health. So singular a *lusus naturæ* is worthy of being particularly noticed; and we hope

that an opportunity will one day offer of ascertaining all the facts relative to this oddly-placed heart, by dissection.

The question, "Is the Yellow Fever always an imported dis-"ease in these States, or may it be generated in them by local "causes?" has lately excited much public and medical attention and discussion.—Dr. David Hosack, the Professor of Botany and Materia Medica in Columbia College, has lately given notice to the public, that, after having collected the material evidence on this subject, he has given over the papers to his pupil, Mr. John De Normandie Gillespie, who has chosen this question as the theme of an Inaugural Essay, which is shortly to be published. In this Essay, we understand, it is the expectation of the author to establish the opinion, that the Yellow Fever is always an *imported disease*.

Dr. Currie, of Philadelphia, is publishing a series of letters, in support of the same doctrine, addressed to Mr. Benjamin Wynkoop, in the public papers of that city; and Mr. Webster, of this place, replies to Dr. Currie, also in a series of letters, published in the Commercial Advertiser, a paper conducted by the last-named gentleman.

We learn that Professor Barton, of Philadelphia, who has lately made a tour through the western parts of the States of New-York and Pennsylvania, intends to favour the public with a Journal of his excursion. This is the more desirable, as we understand that a number of new things have occurred to him in mineralogy and botany, zoology, &c.

FOREIGN.

The following Extract of a Letter from John Marsillac, (a Member of the Society) is translated from the Journal of the Philomathic Society, &c. (Bulletin de la Société Philomatique, &c.) for October and November, 1792. The Facts appear to have occurred in England; but we have not observed any mention of them in the English periodical Publications.

"A peasant of the county of Essex, seeing a great many chil-"dren carried off by the *natural* small-pox, was desirous of inocu-"lating his two boys; one nine, and the other twelve years old. "Not being able to employ a surgeon, he collected the scabs of a "child then sick of the disease, powdered them, and sprinkled the "powder upon slices of bread and butter. The two sons ate them,

" and gave a bit to the *house-dog*. They had a mild small-pox, and " got well without any remarkable accident. The dog remained " sick for two or three days, drinking a great deal, and refusing " to eat; on the fourth, he had a very decided variolous eruption; " on the ninth the pustules were full ripe, and dried up and fell " off, like those of the two children. An English author says he " has observed the same epidemic in a flock of *sheep*, the greater " part of which were affected, and communicated it to two *cows*, " one of which died. The symptoms that manifested themselves " in these animals, in the course of the disease, were in every re- " spect the same as are observed in the human species."

A paper, as we are informed, has been read at one of the late meetings of the Royal Society of London, containing a course of experiments on the combustion of the DIAMOND. It has long been known that this was not an *earthy*, but an *inflammable* substance. But it was not known until now, that this *precious gem* could be converted, by aid of oxygenous air, into carbonic acid gas. This, however, is found to be the fact; and scientific men are led to conclude this brilliant and rare substance to be neither more nor less than *chrystallized charcoal*.

Extract of a Letter from Dr. Beddoes to Dr. Mitchill, dated Clifton, June 15, 1797.

" We seem to have discovered a sure remedy for *diabetes*. It " has answered in three cases of *diabetes mellitus*. Abstinence " from vegetables, and a few drops of *hepatized ammonia*, reduced " the quantity of urine, in two cases, with great rapidity. In one, " volatile alkali only, without absolute abstinence from vegetables, " answered; but the Hot-Well water *(Bristol)* was taken in this " case, and could not be avoided on account of the prejudice of the " patient.

" But I think you will find abstinence from vegetables, with " volatile alkali, either unimpregnated or impregnated with hepa- " tic gas, subdue the most obstinate cases.

" Dr. Rollo, of Woolwich Hospital, is going to publish on " this subject. To him the application of hepatized ammonia " is due.

" I have between 2 and £ 3000* for my Pneumatic Hospital, " which I shall establish forthwith. The observations yet made, " in this country, are partly too few, and partly too inaccurate."

Dr. Rollo's work, we observe by the late London papers, has since been published, in two vols. 8vo; and we hope shortly to have the satisfaction of laying before our readers a more detailed account of his discoveries.

* Sterling.

To the publication of Dr. Rollo, noticed above, is annexed an account of experiments by Mr. William Cruikshank, on the efficacy of *nitrous acid* in the cure of *syphilis*, agreeable to Mr. Scott's method, (See Med. Repos. No. 1. Append.) which we understand are very favourable to that remedy. Indeed, trials seem to be multiplying in various parts of Great-Britain. Dr. Beddoes is now printing, or has already published, a collection of "Reports of the "Effects of the Nitrous Acid in the Venereal Disease." In a letter to Dr. Mitchill, dated Aug. 22, 1797, inclosing the first sheets of the (then) unpublished work, he says, "I send you, by Mr. Garnet, "a proof of the vigour with which the experiments on the anti-"syphilitic powers of the septous acid have been prosecuted in this "country. It appears, even in respect to its certainty, a medicine "superior to any mercurial. Nevertheless, I have collected the "account of some failures, without being able to say on what "the difference of the result depends: a case so common in medi-"cine."

That portion of Dr. Beddoes' reports which has been received, consists of twelve cases, in which the acid effected a cure in the Royal Hospital at Plymouth, reported by Mr. Hammick, jun. a surgeon in that establishment. These are followed by a letter from Dr. Beddoes to Dr. Geach, senior surgeon of the Royal Hospital at Plymouth, whose answer corroborates Mr. Hammick's statements. Some further information is given on the subject, in another letter from Mr. Hammick. The remainder consists of five cases reported by Mr. Sandford, surgeon at Worcester. We confine ourselves, at present, to presenting a copy of Mr. Hammick's letter to Dr. Beddoes, which commences the collection. Should a more leisurely examination convince us that further extracts will interest our readers, and the subject not have grown too common, we shall insert some of the most valuable papers in our succeding numbers. The letter is as follows.—

"*Royal Hospital, Plymouth, July* 22, 1797.

"SIR,

"I do myself the honour of acquainting you that, in consequence of your very ingenious publication on factitious airs (Parts IV. and V.) and your recommendation of the nitric acid in syphilis,* I have been induced to make trial of the medicine in that disorder, under the inspection of my friend, Dr. Geach, who is the senior surgeon of this hospital; and to him I beg leave to refer for the confirmation of the faithful statement of several cases, and the authenticity of the facts.

* For the papers here referred to, see the Appendix to the present Number of the Medical Repository.

"Further—Dr. Geach has been so obliging, as to say he will answer, as far as he can, any questions relative to this subject.

"As I am one of the assistant surgeons under him, and many of the venereal wards are in our department, I had an opportunity of selecting the *worst* venereal cases that were brought into the Royal Hospital. The cases in which the nitric acid has been given already amount to more than fifty in number; and the success has fully answered the character which has been given this newly discovered remedy, which effects a cure, for the most part, in a short time, without impairing the appetite, without requiring any precise or particular regimen, or strict confinement, leaving none of those ill-consequences, or depredations, that often follow the liberal use of mercury.

"The nitric acid does not affect the gums, fauces, or intestines, by exciting vomiting, diarrhœa, or pain, unless the medicine be swallowed hastily, and in large draughts at a time; when thus taken I have known that it has produced a nausea, or mawkish, disagreeable sensation in the stomach, and set the teeth on edge by its acidity; all which inconveniencies have been prevented by enjoining the patient to drink the medicine through a narrow glass tube.

"Not to multiply instances, and tire you with a long detail, I beg leave to submit to your inspection and consideration twelve cases, with a request that you will be pleased to publish them, as they corroborate your own observation: and the publication under your authority may extend the use of the nitric acid, in a disease so varying, that few constitutions can withstand the power of two poisons, the disease and the mercury, operating at the same time. Future experience, however, must determine, whether the cures wrought by the nitric acid will be permanent or not; but if it be permitted to draw conclusions from the progress of the several ulcers, the healthy appearance, and the firmness of the granulations, the softness of the parts, and the cicatrices, little doubt can be entertained but that the nitric acid will, in a great measure, supercede the use of mercury in the venereal disease; and, as yet, I have seen no instances of the disorder re-appearing after the use of this medicine.

"I have the honour to be,
"Sir, &c. &c.
"STEPHEN HAMMICK, jun."

Dr. Carmichael Smyth's mode of preventing or stopping contagious diseases on ship-board, by nitrous fumigation, is now generally adopted throughout the British navy. This discovery has been also taken up lately by the Court of Spain, which has ordered the translation of the doctor's treatise into the Spanish language,

and directed a similar process to be observed in the Spanish marine. [*Monthly Magazine.*

A correspondent of the Norfolk Chronicle recommends as a means to restore infectious air to purity, to wet a cloth of any kind in water mixed with *quick-lime*, and to hang the cloth so steeped in a room till it become dry; after which to renew the operation as long as appears needful. [*Ibid.*

Mr. Nicholson, well known by his "Introduction to Natural "Philosophy," "First Principles of Chemistry," &c. has commenced the publication of a periodical work, entitled "A Journal "of Natural Philosophy, Chemistry, and the Arts: illustrated with "Engravings." It is published monthly, and in 4to. We have received the two first numbers, for April and May last, which contain about 50 pages each, and give reason to expect that the undertaking will be ably and spiritedly conducted. The Journal consists of original and selected essays.

Professor Reich, of the University of Erlangen, in Germany, writes, that several of Dr. Mitchill's pieces on pestilential fluids have been translated into German, and published in the Literary Journals of that country. In pursuance of Dr. Mitchill's original idea, this principle has been applied to explain the distempers of domestic animals, particularly a very violent disease which raged among the cattle in Germany last year. Two tracts have been published on this subject, Reich's Belehrung fur den landman uber die Rindvielseuche (Information for Farmers, concerning the very pestilential Distemper among Cattle,) and Albert's Dissertation de luis Bovillæ origine et Natura, which we hope soon to receive.

From the similarity of constitution between man and quadrupeds, it might be supposed a priori, that pestilential fluids would have very much the same operation on both; and this idea receives strong confirmation from the distempers among cats, dogs, horses, and neat cattle, of which we have received information from different parts of the country. It is much to be wished, that correct statements of facts, relative to the disorders of these and all domestic animals, could be procured, especially when they occur as in the present year, both before and during the breaking out of pestilential diseases among mankind. We hope our friends in the country will engage in this investigation, that we may be enabled to judge how far these cotemporaneous events are deducible from the same principle or referable to the same cause.

MEDICAL OBITUARY.

DIED. At Portsmouth, (N. H.) lately, Dr. Hall Jackson, an eminent physician of that place. His death was occasioned by his being thrown out of his chaise.

At Providence, (R. I.) Dr. Ephraim Cumstock, of the Yellow Fever.

At Jamaica, (L. I.) Dr. William M. Johnson, a native of Massachusetts, but lately from South-Carolina, where he had settled as a practitioner of medicine. He died Sept. 21, 1797, of a complication of disorders, originating in a fever contracted in Carolina.

At Philadelphia, of the Yellow Fever, Dr. Nicholas Way, Sept. 2.—Dr. Jacob Thompson, August 29.—Dr. Samuel Jones.—Dr. Annan, Oct. 4.—Dr. Dobel,—and Dr. Samuel Pleasants, Oct. 15, 1797.

At Baltimore, Dr. Edward Johnson.

At Charleston, (S. C.) Dr. Prentiss, originally from Massachusetts.

FOREIGN. In Swisserland, within the present year, the celebrated Dr. TISSOT, universally known by his professional writings. Dr. Tissot survived his friend ZIMMERMAN (whose death was noticed in the first number of the Repository,) long enough to publish a history of his life, which has since been translated into English.

APPENDIX.

DOMESTIC.

Concerning the Use of Alkaline Remedies in Fevers, and the Analogy between Septic Acid *and other Poisons; in a Letter to* THOMAS PERCIVAL, M. D. *&c. of Manchester, from Dr.* MITCHILL, *dated New-York, January* 17, 1797.

ONE of my friends, who lately returned from Philadelphia, informed me, that some time ago Dr. PRIESTLEY had forwarded to me a copy of his "Considerations on the Doctrine of "Phlogiston." I have to regret, that the pamphlet never reached me. Had it regularly come to hand, I should not have omitted to acknowledge the favour long before now. I have, however, procured a copy since, and perused it with care. It pleases me to find him strenuously urging the supporters of the antiphlogistic doctrine, to prove rigidly, by experiment, every thing they assert; to repeat doubtful experiments again and again, and to reconcile to their system a number of facts which appear to stand in formidable opposition to it. All that he states relative to the constitution of *metals*, to the composition and decomposition of *water*, and concerning *finery cinder*, requires unquestionably further inquiry and consideration. In particular, what he affirms about the formation of *highly phlogisticated nitrous acid*, by taking an electric spark through a *mixture of dephlogisticated and inflammable airs*, is so materially different from what almost the whole philosophical world has of late allowed, that the establishment of it will give a new aspect to things, and overturn much of their reasoning. Messieurs BERTHOLLET, DE LA PLACE, MONGE, MORVEAU, FOURCROY, and HASSENFRATZ, have now a fair opportunity of solving those difficulties, and convincing him of their reconcilement with the New System. For myself, I am one of those who, like BLACK and KIRWAN, changed the phlogistic hypothesis for the antiphlogistic theory, and thought myself a gainer by the bargain.

The inquiry, in which I am engaged, concerning the generation of pestilential fluids, and their action upon the bodies of animals, has led me to examine, to a certain extent, the composition of saline remedies, and their probable mode of operation in the dis-

tempers called "FEBRILE;" and to trace the connection and analogy between "fevers," and other disorders caused by *venomous* fluids. If, by adopting the French nomenclature, and by building upon facts, which have been said to be firmly established, I have been led into any mistakes, either of language or science, they will, I trust, be found to interfere with some part of my *particular reasoning* only, but not to militate at all against my *general conclusion.*

If it is true, that the fluids which, combining with pot-ash, form salt-petre, and with lime, calcarious nitre, are not of mineral origin, but are commonly produced from animal substances, or vegetables that resemble them; if they are not of the putrid kind, but, on the contrary, are highly antiseptic in their operation; and if, at the same time, those antiseptics can speedily destroy life by inducing the worst of symptoms, then they ought, in strictness, not only to be considered as *poisons*, but as *animal* or *vegetable poisons.* And all the *fevers*, as they are termed, of the *malignant*, *putrid*, and *pestilential* forms, are, of course, neither more nor less than the effects of these poisons upon the bodies of animals. Whoever, therefore, falls sick of a plague, or a yellow fever, or any other disease of the same group, should, in proper language, be spoken of as *poisoned*, and treated accordingly.

To him who has perused the preceding papers which I have published on this subject, it will probably appear, that the form which this animal poison takes on, in ordinary cases, is that of an oxyd or acid of septon. Though it becomes me here to observe, that I like not the common way of distinguishing these things into animal and vegetable, because such an arrangement has very little precision and correctness in it, and because the analysis of much vegetable matter shows it to be so similar in its composition to that of animal, that, whatever obvious and superficial differences may appear between the one class of bodies and the other, there can hardly be a distinction in science between the two. And the reason of my dislike will farther appear from what I shall hereafter observe to you.

To a large class of the effects of these poisonous fluids, the term "fever" has been applied. And as such fevers have constituted a great part of human maladies from the beginning, it might be expected that, by this time, their causes would have been thoroughly investigated, and their treatment exactly understood; but this is far from being the case. The misapplication of names has obscured this department of knowledge with clouds and darkness. The discordant opinions of physiologists and physicians, two of whom can scarcely be found to agree, shew, that the rays of science have hardly begun to illuminate or organize this mass of rudeness, after the experience of two thousand years. I question much, whether those venomous or pestilential complaints are better understood, or more successfully treated, than in the days of *the sage of Cos.*

On examining the history of medical practice in "fevers," that is, in cases where those poisonous substances stir up more or less of commotion in the heart and blood vessels, the stomach and bowels, &c. &c. it appears there is no point in which prescribers so much agree as in the administration of alkaline remedies. While contention and strife divide practisers at consultations, whether the alexipharmic, or the depleting plan shall be adopted, they seldom differ much about the administration of soluble tartar, a saline mixture, or some of Mindererus's spirit.

As these remedies enter very deeply into the practice of almost all parties, and have long maintained their reputation, it is very interesting to philosophy and science, as well as to the art of physic, to detect the reason why almost every body agrees in the use of them, in some form or other. By *alkaline remedies*, I mean not merely the pure or caustic alkalies, but all such compounds of other bodies, with alkaline bases, as are capable of being decomposed by the septic poison or acid they may meet with in the human body, and of furnishing an alkali to unite in such cases with these sour and venomous fluids, and thereby to saturate them.

From the class of such substances as yield their alkalies to septic acid, are taken many of the most frequent and popular remedies for "fevers;" and that the whole of them may be exhibited to view at once, their composition understood, their names rectified, and their manner of operating be explained, I have thought it proper to enumerate the greater part of them in the following catalogue: I shall exhibit them under two divisions, of salts and earths; *and first, of alkaline salts.*

I. POT-ASH.

1st. By itself.

Common and old Names.	*Scientific Names.*
Common Caustic, weak and strong, Potential Cautery, Sharp Salt of Tartar, Nitrum Fixatum,	Pure or Caustic Pot-ash.

2d. With Water.

Oleum Tartari per Deliquium, or Ley of Tartar, Lixivium Saponaceum, or Soap Ley,	Solution of Caustic Pot-ash in Water.

3d. With fixed Air, or Carbonic Acid.

Mild Salt of Tartar, Salt of Wormwood, Common vegetable fixed Alkali, &c.	Carbonate of Pot-ash.

4th. With Acetous Acid, or Vinegar.

Regenerated Tartar,
Terra Foliata Tartari,
Sal Diureticus,
} Acetate of Pot-ash.

5th. With Acid of Sulphur, or Vitriolic Acid.

Vitriolated Tartar,
Universal Digestive Salt,
Sal Enixus Paracelsi,
} Sulphate of Pot-ash.

Some particulars concerning the partial decomposition of vitriolated Tartar, by nitrous acid, may be seen in BERGMAN'S Dissertation (§ 9), where it is declared, that after adding powdered vitriolated Tartar to diluted nitrous acid, spirit of wine precipitated a powder from the liquor, which proved to be *real nitre*.

6th. With Citric Acid.

Effervescing Mixture, with Juice of Lemons, &c.
Common Saline Mixture, &c.
} Citrate of Pot-ash.

7th. With Sulphur.

Hepar Sulphuris—Liver of Sulphur } Sulphure of Pot-ash.
Sal Prunellæ } Sulphureo-Septite of Pot-ash.
Sal Polychrest,
Salt of many Virtues,
} Pretty much the same with Sal Prunellæ, though approaching nearer to Pot-ash.

8th. With Oil, Fat, Tallow, and Resin.

Sapo Tartareus,
Soap of Tartar,
Turpentine Soap,
} Terebinthinate of Pot-ash.

Sapo Amygdalinus,
Almond Soap,
Sapo ex Oleo Olivarum,
Castile, Venice, or Alicant Soap,
Sapo Communis,
Tallow, or Slush Soap,
Black Soap.
} Oleates of Pot-ash.

9th. With Acid of Tartar.

Common Tartar,
Tartarum,
} Acidulated Tartrite of Pot-ash, mingled with Carbone and other foreign matter.
Cream of Tartar,
Cremor Tartari,
} Acidulated Tartrite of Pot-ash, dissolved in water, boiled and skimmed.
Crystals of Tartar,
Crystalli Tartari,
} Cream of Tartar re-dissolved and chrystalized.
Tartarus Tartarizatus,
Soluble Tartar,
} Neutralized Tartarite of Pot-ash.

10th. With Muriatic Acid.

Sal Febrifugus Sylvii } Muriate of Pot-ash.

II. SODA.

1st. By itself.

Nitre of the Ancients,
Natron,
Natural Fossil Alkali,
Marine Alkali,
} Pure, or Caustic Soda.

2d. With Water.

Lixivium Natri—Solution of Caustic Soda in Water.

3d. With Carbonic Acid.

Mild Natron—Soda,
Salt of Barilla,
} Carbonate of Soda.

4th. With Ashes, &c.

Kelp———Impure Soda.

5th. With Phosphoric Acid.

Phosphorated Mineral Alkali,
Soda Phosphorata,
} Phosphate of Soda.

6th. With Muriatic Acid.

Common Salt,
Sea Salt,
Culinary Salt,
} Muriate of Soda.

7th. With Boracic Acid.

Borax———Borate of Soda.

8th. With Oils, Fats, &c.

Various Soaps, &c.———Oleates, &c. of Soda.

9th. With Tartaric Acid.

Rochelle Salt,
Sal Rupellensis,
Soda Tartarisata,
} Tartrite of Soda.

III. AMMONIAC.

1st. By itself.

Caustic Ammoniac,
Volatile Ammoniacal Salt,
} Pure or solid caustic Ammoniac.

2d. With Water.

Spirit of Sal Ammoniac,
with Quick Lime,
Eau De Luce,
Aqua Ammoniæ Pura.
} Fluid Caustic Ammoniac.

3d. With Carbonic Acid.

Mild or acrated Volatile Alkali,
Spirit of Hartshorn, &c. &c.
Aqua Volatilis Cornu Cervi,
} Carbonate of Ammoniac.

4th. With Acetous Acid.

Aqua Ammoniæ Acetata, Spirit of Mindererus, } Acetate of Ammoniac.

5th. With Muriatic Acid.

Common Ammoniacal Salt, } Muriate of Ammoniac.

In this long list of alkaline substances, which enter so largely into medical prescriptions in febrile diseases, I have intentionally omitted the combinations of pot-ash, soda, and ammoniac, with the septous and septic acids; and I have taken no notice of emetic tartar, cuprum ammoniacum, and some other alkaline preparations, where the virtue of the remedy resided less in the small quantity of alkali than in the antimony, copper, or other substance with which the alkali was united. And Glauber's salt is omitted, because the septic acid cannot decompound it. What I wish particularly to observe is this, that all the articles of the Materia Medica herein enumerated, are either pure alkalies, or are combinations with other substances, for which the alkalies have a weaker attraction than for septic acid, and are consequently ready to join the septic acid wherever they meet with it, and to saturate it.

Secondly. *Of Alkaline Earths.*

I. LIME.

1st. By itself.

Quick Lime, } Pure or Caustic Lime.

2d. With Water.

Lime Water, } Solution of Quick Lime in Water.

3d. With Carbonic Acid.

Chalk,—Creta Preparata, Crab's Eyes, Prepared Coral, Calcined Oyster Shells, &c. } Carbonate of Lime.

II. MAGNESIA.

1st. By itself.

Calcined Magnesia, } Pure Magnesia.

2d. With Carbonic Acid.

Common or Effervescing Magnesia, } Carbonate of Magnesia.

These absorbent earths will also combine with septic acid in all circumstances favourable to their union, and constitute septites of lime (calcarious nitre), and of magnesia, (nitrous magnesia.)

If any of these substances, whether salts or earths, are prescribed, and, on being swallowed, do meet with septic acid in the stomach or intestines, the necessary consequence is, that their basis will quit the weaker, and join the stronger acid. The prescription

of these things then is, in fact, under such circumstances, the prescription of alkalies. Few of the prescribers, I believe, calculate how many alkaline remedies they order for their patients, and fewer still have any tolerable idea of their real manner of operating. It is amusing enough, that they, who, in their *conversation*, doubt or deny the truth of the conclusions I have made concerning these sorts of distempers, are all the time in the habit of verifying them *in their practice*, and bearing witness for them daily by their prescriptions.

Some of the skilful and knowing ones tell me gravely, I am prescribing for a *phantom*, for a creature of my own imagination, and that nothing can be more remote from common sense, or even probability, than that a man should distil salt-petre in his belly, and carry about the aqua-fortis in his intestines. Whence comes the nitre in the alimentary canal? Where is the stronger acid to decompose it? Who, in his right mind, could ever think of comparing the human stomach to a retort? The answer to all these pretty questions is very short:—That without a distilling process, or a retort, or nitre, or a stronger acid, the stomach and intestines may acquire their septic liquids from the same sources whence pot-ash gets them to form common salt-petre; that is, from the putrid substances, whether animal or vegetable, which, in the first instance, evolve them.

All that is now necessary to show is, that such putrid substances do occasionally exist in the alimentary canal, and that the septic fluids are generated by them. According to my judgment, there can be little doubt of the production of septic acid in the human mouth, from corrupting remnants of food, of its operation in incrusting and corroding the teeth, vitiating the taste, mingling with the saliva, nauseating the stomach, &c. (Letter to Professor HOPE, of Edinburgh.) And that the articles of human food may afford it, appears further from other experiments. For instance; there can be no doubt of its production from putrid blood, whose vapours, in CHAPTAL's experiment, were capable of converting chalk to calcarious nitre. (Elements, &c. article SODA.) And, by trials not more liable to objection, did THOUVENEL prove the existence of septic acid, in the exhalations from *other* putrifying animal substances.

The blood and muscular parts of animals, thus, by putrifying in open air, afford septic vapours, which, when condensed and collected, turn out to be septic acid.

Now, as man is a carnivorous animal, and both blood and muscular solids are frequently taken into his stomach, wherefore comes it to pass, that septic acid is not always produced from a mass of dead animal matter exposed to heat, moisture, and air, as the contents of the stomach are? They who are acquainted with

the functions of that organ know, that in its healthy state, the putrefactive tendency is checked by the gastric fluid, which dissolves the soluble part of the food, and makes it assume a form exceedingly different from putridity. It hence follows, that as long as the stomach prepares good concoctive juice, in sufficient quantity, so long will the contents of it be prevented from undergoing that process whereby septic acid is formed.

But when, from intemperate drinking of hot spirits, or of cold water, from excessive exercise, or too great heat, or from any other cause, the healthy functions of the stomach are imperfectly performed or intirely cease; then food, of the above-mentioned kinds, will corrupt for want of a sufficiency of good gastric fluid to dissolve or concoct it; and the production of septic fluids will, of course, be the unavoidable consequence.

As long, therefore, as a man can keep up his digestive powers, so long will he be free from the internal causes of intestinal "fevers," be his food and diet what they may. Whilst the menstruum which the stomach prepares, is sufficient, in quantity and quality, to perform its proper work, so long will indigestion and its putrescent consequences be guarded against. There would seem, therefore, to be two unpleasant consequences resulting from a weakened stomach; *to wit*, 1st. the corruption of certain articles of diet, within that hollow viscus; and, 2dly, their immoderate retention in a part of the alimentary tube, possessed of exquisite sensibility, and intimately associated with every part of the body. By corruption and detention, the food and morbid humours form frequently as foul and pestilential a collection as any puddle the street contains. Is it wonderful, that flatulency, anorexia, nausea, black vomiting, gastritis, &c. are the frequent consequences?

Is it not wonderful, considering the manner in which *Americans* live, that internal distempers from this cause are not more frequent and deadly?

This indeed would happen, had there not been a secretory bowel in the body of a large size, and important office; I mean the liver, which prepares a considerable quantity of an alkaline liquor, the bile, capable of saturating any small excess of acidity that may prevail in the chyle, by its seasonable and wholesome admixture. The gall-bladder is remarkably and conveniently provided with a residuary portion of this health-preserving fluid, which corrects, as far as it can, any sourness in the alimentary mass, after it has undergone the solvent operation of the gastric juice.

Truly, this secretion of the liver, which some physiologists have treated as an excrement, and others considered as the cause of a vast amount of "febrile" mischief, appears to be one of nature's grand preventives, or antidotes, of the very evils it has been accused of producing.

Mistaken notions have too long prevailed, to the shame of physic, concerning the use and operation of the gall. Its alkaline qualities show how well it is calculated to quell a redundant acid in the first passages. The yellowness of the skin, in certain pestilential diseases, can be better accounted for from the influence of septic acid striking such a colour as it acts upon the skin than from the presence of the bile; and the considerable quantity of it secreted in some of the cases of poisoning called fever, shows not that it is the cause of the disease, but that a copious flow of it has prevented, in the intestines, the fatal consequences of much SEPTIC VENOM produced there. The greenness of the bile, in such cases, is a proof of its mixture with an acid: and, if further evidence is wanted of its salutary and preserving power, I pray you to examine the dissections of bodies dead of YELLOW FEVERS, &c. and satisfy yourself, that as far above and below the orifice of the ductus communis choledochus, as the biliary fluid extends, so far the duodenum and continuous parts of intestine are free from inflammation and its consequences. As to the *artificial* discharges of bile, provoked by frequent and injudicious emetics and cathartics, I need only to refer you to RICHTER's Observations, (chap. xvii.) where this secreted humour is rescued from much of the blame and obloquy that had, for years, been uttered against it.

There would be great ease in bringing to your recollection the sour belchings and vomitings, and a variety of other inconveniencies, besetting the parts between the lips and the pylorus; and it would be just as easy to enumerate the dysenteric symptoms excited by the same morbid cause in the large intestines, between the lower part of the jejunum and the extremity of the rectum, from rotten and feculent matter lodged there, after the strength of the bile was exhausted. But I fear the enumeration of all these symptoms in detail would be tedious, since, if the presence of septic acid in the stomach and colon is admitted, its inflammatory, corrosive, excoriating, and dangerous effects, will, at once, be suggested to your mind. And does not the accompaniment of dysentery with *fevers*, both of the intermittent and remittent types, and their alternations with each other, throw great light upon the whole of these morbid phenomena, and prove them all to depend upon the same common cause?

But here I know it will be objected to me, that my reasoning is all wrong. The old story of the *alkaline products* of putrefaction will be told over, and acids and other "antiseptics," as they are called, must be poured in, to check or correct this alkaline tendency which is held so peculiarly to attend putrefaction. The volatile alkali, it is affirmed, is set loose, and acids must be given to overpower it.

The most valuable and efficacious *salts* and *earths* in my catalogue, are combinations of alkaline bases with acids; and on their decomposition it is alledged their acid, and not their alkali, allays the troublesome symptoms, and relieves the sick from their distress. Peace be to such reasoners! The reply to their objections is very concise, and may be summed up in few words—*that universally, and without a single exception that I remember, all acids have a stronger elective attraction for pot-ash and soda than for ammoniac; consequently the volatile alkali, if it existed there, could not decompose a single neutral salt that has a fixed alkali for its base.* Such persons as hesitate to take this declaration upon my authority, may, perhaps, find satisfaction in examining the first twenty-five columns of BERGMAN's second table of chemical attractions. And as to *ammoniac*, the advocates for the alkalescency of putrefaction should never administer it at all in intestinal fevers, as thereby they must, upon their own principles, augment the disease, and make every symptom worse. And the absorbent earths, *lime* and *magnesia*, even in their aërated forms, must be wretched prescriptions in such cases, if administered only for the sake of the carbonic acid gas they may afford.

Upon the whole then, the gastric liquor and the gall may be considered as the two great opposers of putridity in the stomach and small intestines. When these fail to perform their respective functions, a diet in which much septon, or principle of putrefaction prevails, may turn to the oxyd of septon, or to septic acid. When, in such cases, the substances enumerated above are administered, they act as alkaline salts; and while the weaker acids are set loose, the septic acid turns them respectively into septites of pot-ash, soda, ammoniac, lime and magnesia. If there is no septic acid in the alimentary canal, the neutral salts in my catalogue undergo no decomposition, and exert their several respective sudorific, cathartic or diuretic powers without interruption.

Look now, after this explanation of these matters, into the books, and examine what the febrifuges are. FULLER's *Body of Prescripts*, a work published near the beginning of the eighteenth century, by a Cambridge physician, which lies now at my elbow, furnishes several receipts of this kind, with comments. He affirms (*Pharmacopœia Extemporanea*, p. 63.) "that a *decoction*, made by boiling eight scruples of salt of wormwood, (carbonate of pot-ash) and four ounces of white sugar, in a quart of water, was an effectual medicine, and especially to be prescribed when a remitting fever would not obey the *cortex*, and began to threaten running over to the party of continual fevers." *Salt of wormwood* and spirit of sal ammoniac constitute (ibid. p. 101.) a considerable share of the active ingredients of the febrifuge *draught*, "which," he says, "gives relief in intermitting fevers, especially in such of them

as invade with cruel cold, shivering, shaking fits, that continue long."—"*Red coral levigated* and *salt of wormwood*, with lemon juice and cinnamon water, forms a *mixture* which wonderfully, (ibid. p. 299.) and almost miraculously, (like a god in a machine as they say) represseth subversions of the stomach, and motions to vomit. I have many times observed, that in continual fevers, miserably afflicting with anguish at stomach, and symptomatic vomiting, more good hath been done with this medicine alone, than with all that I ever could, by anxious study and various trials, find out." Salt of wormwood also enters into the composition of his febrifuge powder. (Ibid. p. 383.)

RIVIERE, or *Riverius*, the French physician, who practised at Montpellier above a century back, was very fond of *pot-ash*, insomuch that he gave the name of "febrifuge" to water impregnated with salt of tartar. (Observat. de Med. Centur. iv. Obs. 97.) His febrifuge *draught* (Centur. ii. Obs. 66.) was composed of vitriolated tartar, (sulphate of pot-ash) in some decoction of wormwood; (Cent. i. Obs. 54.) and his method of restraining vomiting in malignant fever was, by administering a scruple of the salt of wormwood in a state of effervescence with lemon juice. (Ibid. Cent. i. Obs. 15.) Though what he seems to have valued more than all his febrifuges, is a *mixture of calomel and jalap*, which he describes in the following enigmatical terms: "Ce precieux me-"dicament est donc composé d'un triple Hercule elevé a la plus "haute noblesse par douze travaux, auxquels est enfin ajouté un "quatrieme athlete qui, acheve l'accomplissement de l'ouvrage." (Cent. iii. sub finem.) To *mercurius dulcis*, six or seven times sublimed, he gave the name of *Calomelanos*, and often prescribed it in the quantity of a scruple, with half the weight of resin of jalap, or scammony. (Cent. iv. Obs. 97.) What a confirmation of modern practice, by the experience of the French physician, often and often repeated more than an hundred years ago!

There would be no difficulty in collecting other examples of similar practice; and, as I have cited an *English* and a *French* authority, I shall also quote the sentiments of a Scottish writer. CULLEN (First Lines, &c. § 153 and seq.) of Edinburgh, enumerates *neutral salts* among those remedies which, when taken internally, are fit to determine to the surface of the body. He bestows on them great praise for the power of putting an end to the cold stage of fever, and of stopping vomiting. He mentions particularly the composition of an alkali, with the native acid of vegetables, and ascribes more or less of the same virtue to all the neutral salts, not excepting the combinations of ammoniac.

Thus, from the examination of the causes of pestilential distempers, there is good reason to believe they arise from septic acid inhaled from without, or generated within the body, and that this

acid is truly a poison; and from an investigation of the composition of a large class of the remedies successfully administered in such cases, and their manner of operating, there is equal cause to be persuaded, that they are antidotes to this poison, and are so by virtue of their alkaline qualities.

After all that has been stated, I should however be more diffident of the conclusiveness of the argument than I am, if the analogy of other poisons did not give it powerful collateral support. And here, facts from the north of Europe, and the south of Asia, corroborate the inference I have drawn, by their united force.

There is a loathsome disease described by *Chevalier* BACK, (VAN TROIL's Letters on Iceland, Letter 24.) called *Elephantiasis*, liktraa, or scurvy, which is known in *Madeira*, and the *Ferro* Islands, but is particularly distressing to the poor inhabitants of Iceland, and the districts of Abo and Oesterbottn, in Sweden. It is caused by the manner of life and diet of the needy and wretched natives, who, besides exposure to wet and cold, frequently feed upon corrupted, rotten fish, fish-livers and roe, fat and train of whales, and sea-dogs, and congealed and sour-milk, with little or no vegetable provision. One might guess, with tolerable accuracy, what would be the consequence of a long continued diet of this sort; which, to use the phrase of the country, makes those who are infected with it in its highest degree, resemble *a putrefying corpse more than a living man.* Offensive breath, perpetual thirst, strong propensity to sleep, and difficulty of being awakened, shortness of breath, want of animal heat, brown and blue colour of the eyes and lips, &c. joined to an infectious quality, or power of being communicated, in some circumstances, from person to person, shew to what length septic mischief may run, even in those cold countries, and what singular appearances it may wear.

This formidable disease has been cured by HEBERDEN. He gave his patients a little electuary of Peruvian bark and sassafras root, kept blisters constantly between their shoulders, and caused their hands and feet to be rubbed, night and morning, with a mixture, consisting of eight ounces of brandy, *one ounce of ley of tartar, and two ounces of spirit of sal ammoniac.* A five months course of this treatment was successful, after mercury, antimony, &c. had been tried full seven years, without any lasting amendment.

Mr. WILLIAMS has made a statement of facts, (Dissertations on the History and Antiquities of India, miscellaneous pieces, No. xvi.) relative to the cure of persons bitten by poisonous serpents in the East-Indies. It hence appears, that the poison of the cobra di capello, and other snakes, is effectually counteracted, even after symptoms of great danger have come on by the external and internal use of eau de luce, or of caustic volatile alkali, and that with surprising quickness.

You, by this time, suspect, no doubt, that other poisons are acids. There are, indeed, other facts that evince the same thing. If I stopped here, you might justly address me in the words of Solomon, "Go to the ant thou sluggard, consider her ways, and be wise." This insect is furnished with a poisonous liquid, which it infuses into the wounds it inflicts upon its enemies. This animal poison changes the blue colour of violets to red, (2 NEWMAN, &c. p. 324.) and forms a neutral salt with pot-ash. (2 FOURCROY, p. 824.) And it is told, on the authority of Mr. HOOK, that the stings of *bees*, *wasps*, and *hornets*, likewise emit liquids of an acid nature, which those creatures discharge on being irritated; and it is related, on the same evidence, (Micrograph, p. 142.) that the thorns of the nettle plant, (urtica) have sacks, containing a poisonous fluid at their bases, and holes near their tips, through which it is emitted very much after the manner of the fangs of serpents, and the stings of insects. *This* poison will probably turn out to be an acid, and throw light upon other poisons called vegetable. Wherein the formic acid and the poison of nettles peculiarly differ from each other, and from the acid of serpents, and the acid of pestilence, I do not pretend to know. At the same time I think it not improbable, that to whatever cause their small degrees of difference may be owing, *they all have an azotic (septic) basis, and the great traits of their character are the same.*

Your note from Manchester of August 6th, 1796, together with the papers accompanying it, on the subject of the medical and municipal regulations adopted in your great manufacturing town, were very acceptable to me. Distempers of a kind deemed infectious, are now and then prevalent among your people, as well as among the inhabitants of our American cities.

May "the sting of death," which is so often poisoned with an acid, never penetrate you, while your benevolence shall continue to be exerted in bettering the human race—When this ceases, "your corruptible may put on incorruption" (PAUL writes like a philosopher) as soon as it pleases.

Yours, &c. &c.

SAMUEL L. MITCHILL.

FOREIGN.

Additional Account of the Use of NITROUS ACID *in Syphilis. Republished from Appendix, No.* V. *to Part* V. *of* Considerations on Factitious Airs, *by* Thomas Beddoes, *M. D. and* James Watt, *Engineer.*

NOT many days after the receipt of Mr. Scott's paper on nitric acid, the Editor was consulted on a singularly obstinate case of supposed Lues Venerea. Above ten months ago, the patient had slight gonorrhœa; and after the use of some common remedies he thought himself nearly well, when a small ulceration appeared on the *glans penis*. I am not informed whether this ulcer shewed the characteristics of chancre; but, by the advice of an experienced practitioner, he now took the mercurial pill night and morning, and rubbed in mercurial ointment. This course was continued for near eight weeks without any soreness of the mouth, or amendment of the ulcer. In about four months from the first seizure, under the continuance of the same plan, an ulcer appeared in the throat, and the medicines were exhibited more largely for six weeks—and he was at the end of this time rather worse than better. The patient was now confined to his apartment, and mercury used more vigorously in both ways. In the course of five weeks, the ulcer was reduced to a third of its former extent; its pain had subsided, and the gums felt slightly sore. After this time, the chancre (?) was in a fluctuating state, sometimes healing rapidly, and then spreading to its original dimensions; nor was any farther advance made towards a cure, though corrosive sublimate was employed alternately with other preparations of mercury. Eight months after the first attack, there was observed near the sore, a gangrenous spot; having nothing of the appearance of a spreading venereal ulcer, but exactly such as is sometimes seen after long mercurial courses. The mercury was reduced; cicuta applied, and taken with bark and sarsaparilla; a grain only of calcined mercury being given at night, with half a grain of opium.

The Editor produced Mr. Scott's paper, and recommended a trial of his method, to which the patient and his friends cheerfully consented. The nitric acid was accordingly taken with perseverance for six weeks, and for a time to 200 drops a day. Mean-

while the ulcer rather extended than diminished; its surface looked cleaner, but no tendency to incarnation appeared. Immediately on taking the acid, no sensation was felt in the stomach, either pleasant or otherwise, except during the first, second, or third days, when it griped a little—No increased warmth of the skin—no effect on the pulse. The night-sweats, (which were certainly less after the mercury was reduced, and the bark and sarsaparilla employed) declined rapidly, after the acid was begun upon.

The acid being now gradually reduced to eighty drops a day, its inefficacy in this case became too apparent. The ulcer suddenly put on a worse appearance, and increased rapidly. Inflammation of the sound part of the glans, and a degree of phymosis, succeeded.

Opium, with bark, has been since advised, upon the supposition that the ulcer is a mere ill-conditioned ulcer, and now no longer of a venereal nature, whatever it may have been before the employment of mercurial medicines.

DEAR SIR,

I RECEIVED your obliging favour on the day it was written, and perceive by its manner that you are as much and as agreeably surprized by the information Mr. H.'s Letter to me affords, as I expected you would be. I think with you that the Public ought to be made acquainted with the history of his case, together with the contents of his Letter; and as I have personally witnessed what he has recorded, I should have to accuse myself of inhumanity, if I were to suffer any personal considerations, that respect either my time, or my unwillingness to appear so often as a narrator of uncommon cases in one pamphlet, to prevent my furnishing you with all the particulars within my knowledge. I therefore with cheerfulness and pleasure undertake to give you, as far as I am able, with the assistance of my day-book and recollection, the account of a case that you say will conclude your pamphlet, "and make a noble conclusion." But as it may be asked, how I came to be acquainted with the effects of the remedy before its publication, it may not be amiss for me to state that, when Mr. Biggs, your Printer, furnished me with the proof-sheet for revision, that contained the account of Mr. Henderson's case, I was struck with Mr. Scott's communication which occupied the preceding part of the same sheet; and as soon as an opportunity permitted, I informed you, that I had a venereal patient under my care, whose case had resisted mercury in almost every known mode of exhibition, and that I should wish to try the nitric acid as there recommended, and requested that I might be favoured with your company when I visited the patient the next morning, as well to as-

sist me with your opinion respecting the appropriate dose, &c. as to give you a demonstration of the case being almost as bad as ever had been observed by any practitioner. You obligingly promised to be with me at nine in the morning; but about that time I received a note from you, wherein you informed me that a professional hindrance would prevent your being with me, and that upon reconsideration you thought the reasoning of Mr. Scott, who refers the advantages obtained "to oxygenation of the system," so unsatisfactory, that you would advise a very doubtful prognosis to be given to the patient, if he were put upon a trial of it. I was so much influenced by your opinion, that I should have omitted to make use of it, had I not the preceding evening shewn my patient Mr. Scott's account, and told him that he might expect a visit from you in the ensuing morning. A man in his situation, that had for nearly two years experienced the inefficacy of what he had been frequently told was the whole of the known remedies that the art afforded, was not likely to give up the use of that which was asserted to be a tried remedy in another part of the world, though it was unknown in this; and although I made him acquainted with the contents of your note, he was no way discouraged, but earnestly petitioned for its immediate exhibition. I have thus stated, in a plain and candid manner, the way in which I was led to its use. And if there should be any person, who, after what has been said, can suppose that any mistake in the nature of the case, or misrepresentation of the facts, can have happened—or (which is more agreeable to believe) if any gentleman should wish to ascertain, in a more particular manner, the facts that preceded and accompanied the administration of a remedy of so much apparent importance in a disease so hopeless, I am happy to say, that my patient's very laudable humanity would induce him to sink the consideration of what he might feel in satisfying such inquiries, in the hope of assisting, by his confessions, some unhappy fellow sufferer.

On the eighth of February, 1795, I was requested to visit Mr. ———, with pains of the limbs, and ulcers of the throat and tonsils, which, from appearances, suspecting to be a case of cynanche maligna, induced me to prescribe bark, wine, and gargles. That plan was persevered in until the 19th, without any advantage. From that circumstance, conjoined with the situation of his pains, and the times of their aggravation, I began (though my patient was married, and the father of healthy, fine children) to suspect the case to be venereal; and after expressing my suspicions, I learnt from him that he had contracted such a disease some years before, and had at that time an ulcer on the penis; but that he supposed himself perfectly cured, having passed through a regular course of medicine for that purpose: it was with difficulty that I

convinced him that his present sufferings were referable to such a cause. However, possessing his confidence, I prevailed upon him to commence a mercurial course, and he continued to take, from that time to the 8th of March, a grain of calomel, with an equal quantity of the ext. papav. three times a day, in the third part of a pint of the decoct. lignor. cum rad. mezerii. On that day, in consequence of the appearance of some eruptions, the form of the medicines was exchanged for the following:

℞ Hydrarg. Muriat. gr. iv.

Aq. cinnam. ℥ iv. *m* cap. coch. larg. noct. maneq.

This was continued until the 27th of April, and then a drachm of the ung. hydrarg. fort. was ordered to be rubbed into the thighs every night at bed-time, and the former medicine omitted on account of the eruption having yielded, though the pains had increased: this, with the decoction of the woods and mezerion, was continued till the middle of June. On the 20th of October, he began to take four grains of the blue pill, with a quarter of a grain of opium, three times a day; and it was continued, without intermission, until the middle of January, 1796. On the 14th of that month calomel, with opium, was again used in its stead, and continued until the middle of February. On the 12th of July, he again commenced its use, and continued to take it until the middle of August last, when I was obliged, by the fullest experience of the inefficacy of all the mercurial preparations that had been tried, to again request him to desist from the use of all medicines, except occasional opiates to mitigate his pain, which it had been necessary for him to use with the greatest freedom during the whole of the mercurial course. It will here be necessary to remind you, that when I first was called to his assistance I found him labouring under only the constitutional or secondary symptoms of the disease; and as I did not keep any minutes of the case, I cannot speak with that exactness I would wish of the particular effects of the different preparations of mercury that were exhibited. But I have the fullest recollection, that the ulcers of the throat and the affection of the skin were removed in due time by the means that were adopted: and although there was a complete failure in my attempts to dislodge the poison from the bones, I am not convinced that the failure resulted from the incompetency of the mercury to produce such an effect—as the constitution of this patient was rendered so irritable by the disease, or the means made use of, (or perhaps both) that I was never able to impregnate the system with a sufficient quantity to produce the desired effect; though it was exhibited with every precaution, and united with every corrective that my judgment could suggest, conjoined with the advantages of country air, bark, milk diet, and the occasional omis-

sion of all medicines, which, for a time, answered so well, as to make my disappointment the greater at each time of the disease returning. At last, medicine of almost every kind failed to afford even relief, and I was reduced to the necessity of being content with the mere palliative effects of opium given in large doses. The peculiarities of this case are however too common; and every practitioner that is much engaged in a large city, especially if it be a sea-port, must have had to deplore such occurrences. It was my good fortune to meet with Mr. Scott's communication, in the way before described, just at the time when I had exhausted my endeavours to cure this patient—and when I say with him, that he was indeed a "rueful spectacle," with little more than diseased bones remaining, when he began the use of the nitric acid; and that he now appears in good health, I contemplate with astonishment the change that has been produced: more especially as he was always labouring under profuse sweats, diarrhœa, ptyalism, from the mercurials that were exhibited, though they were so guarded; and has now obtained a cure by the use of a most powerful acid, without experiencing even momentary inconvenience, and in less time than would have been requisite to remove even the mildest symptoms of the disease by any other known method. How much will the pleasure, that the effects here related have afforded me, be increased, if the future experience of my professional brethren should coincide with what was observed in this very melancholy case: and that it may prove so in cases that are similar I entertain the greatest hope, as the effects appeared to depend so fully upon the assigned cause, that no doubt can possibly remain upon my mind of the acid being the true and only cause of the changes that followed. It will be recollected that the poison had in this case observed its accustomed laws, and after traversing the system, and successfully displaying its character upon the genitals, fauces, and skin, it had affected the bones; from whence most practitioners have found it difficult, and sometimes impossible, to dislodge it. Perhaps it may not be difficult to conceive that a remedy, possessing such chemical power, and having so strong an affinity for the matter that analysis tells us constitutes bone, may produce a cure in this species of the disease, and not be attended with equal effects where only the soft parts are affected by the poison. That this stage is the most difficult to remove, and that it is often impossible to accomplish it, I have before said, is known to most practitioners. May this remedy supply the desideratum of this branch of the healing art, and the name of your correspondent, Mr. Scott, will then be deservedly remembered by every friend of science and improvement. Why should it be so difficult to believe the existence of other specifics for the poison in Europe, when it is recollected that the South-sea islanders, and

some tribes of the native Americans, have found, in the vegetable productions of their countries, remedies for the worst states of the disease?

I have thus endeavoured to comply with your request, as fully and particularly as the circumstance of my having kept no minutes of the case, will allow me; and as the recollection of the patient will be long kept alive by what he has suffered, I think that his own letter may be as satisfactory as any account from me of the situation he was in, when he first made use of the medicine that has occasioned his cure. And I can safely assert, that I believe his account to be a most faithful representation of his situation. You have seen it in his own writing, and he has been taught to expect a visit from you. I therefore request that you will, at an early interview with him, oblige me by satisfying yourself of the whole of what is here asserted; and then, if it be in time for your pamphlet, subjoin your testimony to that of

Your sincere servant,

Nov. 28, 1796. THOMAS BAYNTON.

To Dr. BEDDOES.

DEAR SIR,

PURSUANT to your request at your last visit, I take my pen to describe (if possible) the deplorable conditions and sufferings I have endured for near two years last past, and the almost miraculous deliverance therefrom, by your care and unwearied attention thereto.——I was first seized with ulcers of my throat and violent pains in my shins at night, that threw me into such perspirations, that for nine or ten mornings I was under the necessity of changing my linen before I could possibly get up; which was soon followed by or with excrescences or nodes from my knees almost down to my insteps, attended with violent pains in my head. My arms also were attacked with excruciating pains, where swellings of a considerable size made their appearance. My knees also swelled, and the pain so acute, that I durst not move them the least aside: sleep fled—nor did it return for ten weeks. And for twenty-two weeks I could not bear to be moved without suffering the most extreme torture, notwithstanding your tender care to administer every thing you could devise and prescribe for my relief. I knew you perfectly understood my case; but my disease seemed to baffle the power of medicine and every effort. Having for the then last thirteen weeks lived wholly upon milk, you advised me to discontinue the medicine, in hopes I might soon be able to make use of stronger food, and recover a little strength.

This treatment had the desired effect: and my pains for some time seemed to abate; but, alas! they soon returned again! when you advised another course of medicine, which operated more powerfully than it hitherto had done, and in a few months restored me so as to enable me to walk from my lodgings in the country to town. The satisfaction you expressed on the occasion I shall never forget, and with myself was in hopes of a radical cure. But at the end of three months, my hopes were destroyed by a violent relapse, which soon confined me to my bed. My legs (if possible) were worse than before; for not only my shins, but the main bones pained me dreadfully. One node formed (a little below my right knee) in a short time almost as large as an hen's egg. The pains from my shoulders to my fingers' ends I can scarcely describe. The sinews of my arms, thighs and legs, stiff and contracted—my fingers I could by no means bend; they were fixed by disease, and every joint swelled. The bones of my head shared equally with the parts I have described; and nothing but death was expected to put a period to such a scene of misery. Added to this, my body was a rueful spectacle, a mere skeleton; so that disease had nothing left but my vitals for its prey. This, Sir, is a faint description of the state and condition you found me in about three weeks ago, when you visited me, and with joy in your countenance told me, a new discovery had been made of a medicine that you had great hope would reach my case; and with your wonted goodness of heart cheered up my drooping spirits, by describing to me its mildness and efficacy in several cases similar to my own. Encouraged by this information, and relying upon your judgment, I was determined to give it a fair trial. I began, and continued to take the quantity as prescribed. —At the expiration of seven days I found it begin to operate, as you had before described, by creating a saliva in my mouth. On that day I had a desire to be lifted from my bed, and to sit up a little, which was done with some difficulty; but could not bear my feet on the ground, my knees also being in a very debilitated state —but found my pains greatly abated. I spit a great deal the next night, which was very thin, and not disagreeable. On the eighth day my pains seemed quite gone; and I requested again to get up, when to my great surprize I found myself capable of bearing the weight of my body on my legs. On the ninth day I was capable and absolutely walked from my bed to my chair, the distance of six feet, without assistance.—I bespoke a pair of crutches, but, thank God, I never used them, nor have had occasion for them; for on the tenth day I walked several times backwards and forwards my room without crutch or stick, or any other assistance whatever. On the eleventh day I walked from one room to another, and finding it attended with no extra pain, but stiffness and weak-

ness in my shins, I absolutely walked up a pair of stairs of fourteen or sixteen steps, and down again. My appetite was now restored to an amazing degree, insomuch that I found I could not continue the usual quantity of medicine (which in fact seemed to have operated more like a charm than a medicine) but I continue taking about three parts in four thereof daily. And I have the pleasure further to inform you, that I have walked out several times, and yesterday in particular I walked more than a mile, and was in hopes to have surprized you (which I know would have been an agreeable one) by paying you a visit at your own house; but was informed you was from home.—Be assured I shall always esteem it a pleasure to answer any queries respecting my case, and the efficacious operation of the acid in so wonderful a cure.

Believe me to be,

Dear Sir,

Bristol, Nov. 25, 1796.

To Mr. BAYNTON.

Nov. 29, 1796. The Editor, in company with another person, met Mr. H. at Mr. Baynton's. Mr. H. by word of mouth, confirmed the preceding statement; and added a variety of particulars respecting his former and present state. He said that his daily dose of colourless strong nitrous (nitric) acid was two drachms, diluted by a quart of water. The mixture produced no immediate sensation in the stomach. He was never griped by it; and he thought it rather astringent; for he had no stool in three or four days after beginning this course, which was unusual with him; but he would not take any aperient medicine, that nothing might interfere with the acid. On the seventh day he perceived a flow of saliva: on the eighth the salivation amounted to a quart; and it has continued in a greater or less degree ever since. There was no soreness of the mouth, or any of the feelings which had formerly been produced by mercury. In two or three days after this effect on the salivary glands, he lost his night pains. On being lifted out of bed, he was astonished to find he could stand without support; "whereas," said he, putting his hand on his knee, "these limbs could not before have sustained a single ounce."—The nodes, which were of the size of a hen's egg on the tibia, and of which there were several on the back part of the humerus and radius of one arm, began to diminish. The Editor, who had this interview three weeks after the first exhibition of the acid, found the tibiæ rough, but without excrescence; all the nodes

having disappeared. Mr. H.'s nose, which had been considerably enlarged, was now of the natural size. His hands, which had been "a mishapen mass," had no preternatural appearance. Before, he could not bend any of his fingers; now he could bend them all, but the forefinger of the right hand. A great difficulty of deglutition had disappeared at the commencement of the salivation. His general bodily condition had been much amended, and his appetite had become so keen, that he could hardly find time enough in the day to eat and to take his diluted acid.

This account fully confirms an important part of Mr. Scott's assertions. And the effect of the nitric acid in the instance of Mr. H. furnishes an additional probability that the first case in this Number was not venereal, when the patient began to take the acid. The result of that case goes some way towards determining the limits of its power, which cannot however be fixed without a number of experiments. The Editor has the pleasure to add, that he has before him a letter from an accurate medical observer, in which he says that "he has used nitric acid with great advantage "in some cases which he believed to be hepatic, and in one venereal "case with apparent advantage."

An ingenious friend had conjectured from Mr. Scott's paper, that nitric acid only renders the system susceptible of the mercurial stimulus. But Mr. H. took no mercury between the middle of August and the second week in November. In the effect too on the mouth Mr. H. remarked a striking difference.

The Editor flatters himself that to have ascertained the mild action of the nitric acid in these two cases, and its effect in checking night-sweats in the first, will, in all likelihood, prove advantageous to medicine in various respects. The personal examination of Mr. H. has put it out of his power to doubt of the utility of Mr. Scott's practice. He reckons his success among the fruits that were to be expected from the cultivation of the pneumatic physiology. He felt from the first that his particular speculations were precarious, and he some time since gave up his supposition relative to the state of the system in consumption. *(See second Edition of parts* I. *and* II. *of Considerations.)* But he was confident that if he could fix the eye and the mind of his medical contemporaries on those agents, upon which the mechanism of life so immediately depends, they would not fail speedily to acquire much of that power, which the knowledge of nature confers.

The discovery of every new *specific* (or substance capable of correcting given morbid actions of the system) affords a new reason for believing in the existence of others. For some scrophulous ulcers we seem to have found a new specific in sorrel: for venereal and hepatic affections, in nitric acid, such as we possessed before in mercury. These are diseases, in themselves not many

degrees less formidable than cancer and consumption. We have analogies enough to persuade us, that there is no lesion of organization, induced by the powers of the living body modified in one way, which the same powers, differently modified, may not repair; and that, by dint of frequent ventures, some happy hand will draw from the lottery of Nature a remedy for each of those diseases, which at present most baffle the physician and torture the patient.